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Applied Physics

IN January, the *Journal of Applied Physics* appeared in a new dress. The enthusiastic reception given it leaves no doubt of the need for a medium of its general character in American physics. The origin of the present form of this journal is described in detail in the report of the Advisory Council on Applied Physics given in this issue.

Those most responsible for the recasting of the journal *Physics* were Dr. M. J. Kelly, chairman of the Committee on Applied Physics of the American Physical Society, Dr. J. T. Tate, chairman of the Governing Board of the American Institute of Physics, who was editor of *Physics* from its beginning in 1929, and Dr. H. A. Barton, director of the American Institute of Physics. The establishment of this new journal is the first step in a broad program to give further recognition to applied physics, which is being carried out by the Institute with the aid of its Advisory Council and also by the American Physical Society.

A further step in this same direction is the preparation of a book on applied physics by Dr. G. R. Harrison, director of applied physics at Massachusetts Institute of Technology. The obvious qualifications of Dr. Harrison insure a book which will do much to stimulate further interest, not only in the applications of physics, but also in the fundamental research which is so necessary if a science, or for that matter a civilization, is to progress.

A third step in this program is the sponsoring of conferences and symposia on various phases of applied physics. The very successful symposium on metals just held at Cambridge, Massachusetts serves as a fine example of the kind of meetings proposed. Another is the symposium on physics in the textile industries to be held at Chapel Hill, North Carolina. Conferences on various other subjects are planned for the future. Many of the papers of the Cambridge meeting will be published in the March *Journal of Applied Physics*, while those from the Chapel Hill meeting are scheduled for the April issue.

Every possible effort is being made to publish in the *Journal of Applied Physics*, in an attractive form, papers which appeal to its readers. If topics of interest are missed, or if a different arrangement is desired, a note to the editor will receive immediate attention.

THE EDITOR.

For March

Biography of Albert Sauveur, by LIONEL S. MARKS

Diffusion in Solid Alloys, by R. F. MEHL

Spectrochemical Analysis of Ferrous Alloys, by H. B. VINCENT AND R. A. SAWYER

Modern Theory of Solids. II, by FREDERICK SEITZ AND R. P. JOHNSON

Some Physical Problems in the Electrical Power Industry, by JOSEPH SLEPIAN

Contributed Research papers on the Plastic Behavior of Metals, Dielectric Loss Measurements, and Re-ignition of an Arc.

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Vibration in *Industry*[*]

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MECHANICAL vibrations have been of interest to the physicist from the very beginnings of the science itself. The Greeks philosophized on the music of the spheres, Huygens conceived light as a vibration, and the first models of the atom a few decades ago were simple vibrators. Apart from these highly important, but somewhat speculative conceptions, the principal practical applications of mechanical vibrations are threefold:

1. *Seismology*, consisting not only of the study of earthquakes, but also of the effects of these disturbances on building structures. Recently the peculiarities in the propagation of waves caused by artificial, local explosions have been used as a successful means of prospecting for oil and minerals.

2. *Acoustics*, which has made tremendous advances lately and to which a whole Society in this Institute is devoting its energy.

3. *Vibrations in machines and instruments*, a broad field of importance to the physicist as well as to his more practical brother, the mechanical engineer.

In this symposium it is only the last of these subdivisions that will be discussed briefly, while seismology and acoustics will have to be passed over.

As a science, our subject is very old; the names of the three patriarchs, Galileo, Newton and Huygens, are intimately connected with vibrations in mechanical systems. Though the general theoretical foundations were practically finished two centuries ago, it is only quite recently that the applications to machinery have attained practical importance.

As late as the year 1894, Lord Rayleigh in the preface to the second edition of his celebrated book, *Theory of Sound*, made the statement: "Without ears we should hardly care much more about vibrations than without eyes we should care about light."

This vividly brings home the fact that in a world where steam engines ran at 150 r.p.m. our subject was hardly of practical importance, but it was very fortunate indeed that by the time the machines did speed up, a complete theory was ready, worked out principally by Rayleigh only with acoustic applications in mind.

The actual history of our subject in its narrow sense starts about with the year 1900, but there is one notable exception, because the very first problem in this category was conceived and solved as 1877 by Routh in England.[1] The familiar flyball governor of a steam engine is a system capable of oscillation, the balls moving up and down at a rather slow frequency. The steam entering the engine is a source of energy, which under certain conditions may make the system unstable so that the amplitudes of the governor oscillations become greater and greater, and the speed of the engine more and more non-uniform. Routh laid the general foundation for

[*] Presented at the meeting of the Founder Societies of the American Institute of Physics, October 29–31, 1936 in New York.

all problems in this class and in particular solved it for the simple flyball governor. On our present-day turbines the governing mechanisms have become very complicated and the stability of operation remains a problem that has to be investigated for each new design.

The first really serious troubles in machines on account of their dynamic forces occurred shortly before the turn of the century, when the sizes and speeds had increased to such an extent that the inertia forces ceased to be negligible. There was first the quadruple expansion steam engine for ship drive which developed to a point where weights of several tons were moved up and down through distances of six feet several hundred times per minute. A satisfactory method of counterbalancing these huge inertia forces was worked out by Schlick,[2] which opened up new possibilities for increasing the sizes and speeds of these engines.

However, no sooner had this theory been evolved than the object of all the calculations fell into disuse and was gradually replaced by the steam turbine. This was a first rate calamity for a beautiful theory, an occurrence which I believe is not unfamiliar to the atomic physicists of our own day.

The successful rival, that is, the steam turbine, had made its appearance on the scene with a curious intellectual misunderstanding. Rankine, the celebrated authority of his day, had it that a thin shaft in two bearings loaded with a disk in the center could not run above its critical speed in a stable manner. The Swedish engineer de Laval claimed it could do so. Both men based their conclusions on imperfect theories, but de Laval proceeded to perform the experiment and constructed a satisfactory turbine that actually ran above its critical speed. After that, it took the combined college professors of the world only two years to develop the correct theory. However, there is still many a physics textbook in use today in which certain implications of Rankine's misunderstanding are handed on to the next generation.

FIG. 1. Fatigue fracture of shaft caused by torsional vibration.

Since 1900, and more particularly since the war, the number of vibration problems in machinery has increased by leaps and bounds. One of the most serious is that of torsional oscillations in the shafting of engines in which the torque is nonuniform, such as gasoline or especially Diesel engines. Fig. 1 shows what was once such a shaft, and the fracture at 45 degrees with respect to the longitudinal direction is typically a "fatigue" break, which can be reproduced in the laboratory by a large number of repetitions of torsion in alternate directions. The torque of a Diesel engine is anything but a smooth function of time. It is great just after an explosion has taken place in one of the cylinders and considerably smaller between explosions. The torque-time curve is periodic but quite irregular and thus contains many harmonics. Moreover, the Diesel engine with all its cranks, flywheels, etc., is a complicated elastic system having a large number of natural frequencies. Whenever the frequency of any harmonic of the torque coincides with any natural frequency of the elastic system, there is resonance. Thus there are several spectra of resonances in the speed range of the engine. Occasional failures of the nature of Fig. 1 occurred for many years and the cause was vaguely understood by some designers, but during the war, with the increased use of the Diesel engine, especially for submarine propulsion, the number of these failures assumed epidemic proportions. They occurred quite impartially on the side of the defenders of culture as well as with the defenders of democracy, but since the submarine at that time was a more cultural than democratic phenomenon, the Ger-

mans had most of the trouble. By 1919 methods had been developed by Wydler[3] and by Holzer[4] which made the precalculation and avoidance of the critical speeds a matter of routine in ordinary cases.

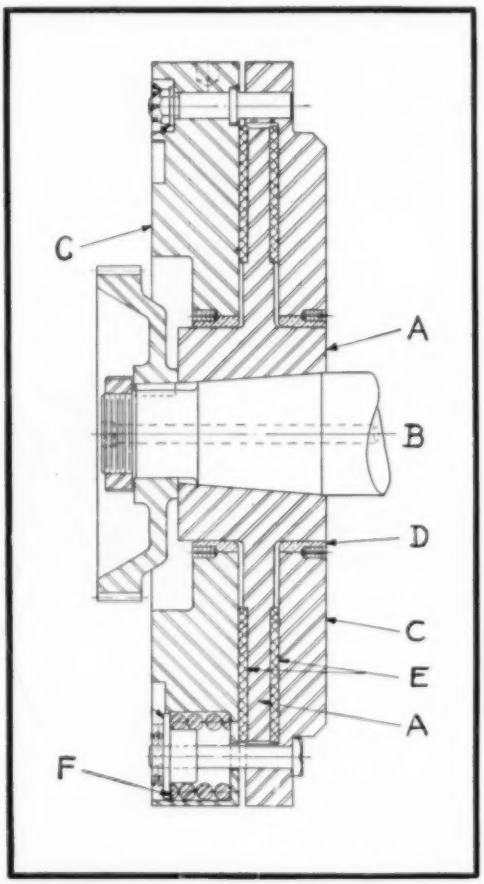

FIG. 2. Lanchester damper.

However, when couplings of a nonlinear torque deflection characteristic exist in the system, it is still difficult to calculate these speeds. This was brought to light in a very dramatic manner by the voyage of the *Graf Zeppelin* on May 17, 1929. Starting from Germany for South America, the ship had been out of port only for some four hours when the first crankshaft broke in two. Several hours later the shafts of four out of the five existing engines had failed and the ship was in a gale. It could be landed only with the greatest difficulty and upon inspection of the only surviving engine a crack was found in the shaft which would have made its failure certain within an hour. Truly a hairbreadth escape. At first it seemed incredible to assume that the engines could have run at a critical speed, because they had not only been calculated but also thoroughly tested. However, it appeared that just before this voyage the ship had been overhauled and on that occasion certain bolts in the coupling of the engine had been "tightened up" to take out "play," which unwittingly had changed the coupling stiffness. The result was that two of the critical speeds of the multiple spectrum fell together, and while each of these resonances individually was known to be nondangerous, their accidental combination proved to be too much. It goes without saying that everybody concerned with these problems learned a valuable lesson from this case.

IF the Diesel engine to be constructed is called upon to operate at one speed only, it is always possible to so design it that no resonance condition exists at the running speed. However, quite often the engine is required to run smoothly over a very wide speed range and then it is usually not possible to make that whole range completely free from dangerous resonances. In such a case the unavoidable operation at a critical speed should be without damage to the shaft and the application of a damper becomes necessary. The simplest of these, invented by Lanchester, is

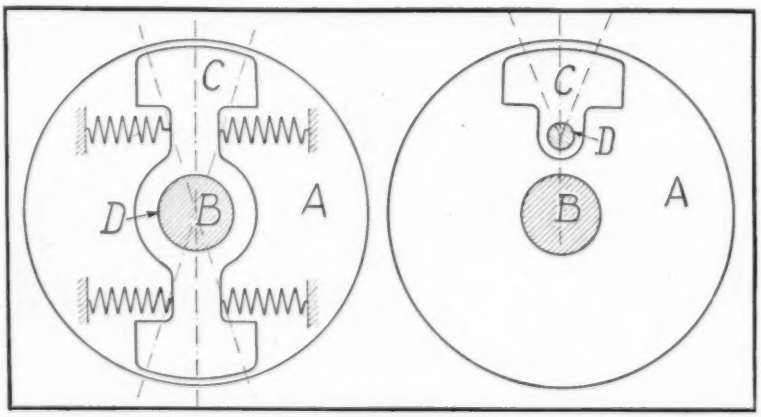

FIG. 3. Nondissipative damper good for one frequency only.

FIG. 4. A pendulum with a frequency proportional to the speed of rotation.

shown in Fig. 2. A disk A is keyed solidly to the end B of the engine shaft, so that A must partake of the torsional oscillation. Two other disks C are loose on the shaft and can rotate freely on bearings D. There is brake lining E between the tight and loose disks and the loose ones are pressed against the tight one by springs F. But for the friction between the disks the outer, loose disks C would rotate at a uniform speed and thus there is a relative oscillation between A and C. The springs are tightened half way, so as to insure sufficient friction, but not so tight as to prevent all relative motion. The energy dissipated in the brake lining due to the relative motion comes out of the torsional vibration and thus cuts down its severity.

Another, nondissipative, damping device, invented by Frahm,[5] is shown in Fig. 3. The meaning of the four letters A, B, C, D is as in the previous figure. However, the loose mass C is not restricted in its motion by friction, but rather by four springs, one end of each of which is attached to C and the other end to A. If A is held still, C has a certain natural frequency of vibration. If the shaft B and with it A executes a torsional vibration at that same frequency, the system C gets into resonance and reacts back on A. It can be shown by an elementary analysis that the reaction is such as to set the system A to rest, i.e., such as to destroy all vibration in A and transfer it to C.* This seems

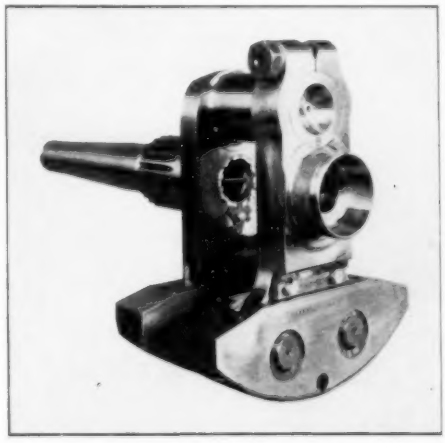

FIG. 5. Application of Fig. 4 to an airplane engine (Wright Aeronautical Corp.).

* This is the mechanical analog to the well-known L–C-wave trap circuit, which, when excited by a finite voltage of its own natural frequency prevents all current in the main circuit.

an ideal damper; but it will operate only at one frequency of disturbance, namely, the natural frequency of the system C.

The torque disturbances in a Diesel or gas engine, however, have a frequency proportional to the speed, and therefore the device of Fig. 3 will operate on such an engine only if the natural frequency of the system C can be made to vary proportionally with the engine speed.

This was done recently by Taylor,[6] who replaced the coil springs of Fig. 3 by the centrifugal field of rotation, as shown in Fig. 4. In this figure the four letters A, B, C and D again correspond to the two previous figures. The loose pendulum C, rotatable about D, will be held in its central position by the centrifugal force, the intensity of which is proportional to the square

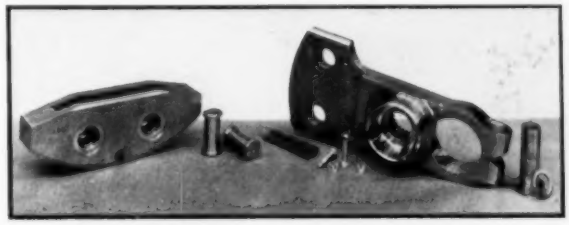

FIG. 6. Parts of the damper, Fig. 5.

of the speed. Hence oscillations of C about its central position will be with a natural frequency proportional to the rotational speed of the engine. As with the device of Fig. 3, a nonuniform rotation of A will set C into resonant vibrations which, in turn, react on A so as to annihilate the nonuniformity of rotation.

The idea has found practical application recently on the Wright airplane engine, as shown in Figs. 5 and 6. Since, for this case, calculations showed that the effective length of the pendulum C should be about 0.1 inch, the construction of Fig. 4 had to be modified, which was done quite ingeniously by R. Chilton.[6] The front crank counterweight of Fig. 5 is loose and contains two holes. The crank itself contains two holes of the same diameter. The two are connected by two pins of a radius 0.1 inch smaller than the hole radii which enables the loose counterweight to swing parallel to itself in a circular arc of 0.1 inch radius. Fig. 6 shows the various parts of the construction. This device has

eliminated the trouble of torsional vibration throughout the entire speed range of the engine.

THE modern steam turbine has given rise to several important problems of vibration. The disks carrying the blades or the turbine

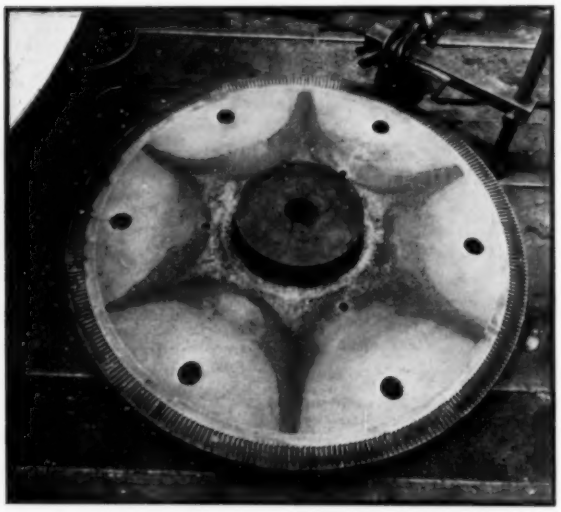

FIG. 7. Turbine disk vibration (General Electric Co.).

blades themselves have shown epidemics of fatigue failures in the past, caused by resonance conditions either with the speed itself or with one of its multiples. Therefore it is of importance to determine the natural frequencies of the various parts. Since the structures involved are usually too complicated to expect sufficient accuracy from a calculation, the frequencies are usually determined by experiment. Fig. 7 shows a turbine disk excited to resonance by an a.c. magnet in the upper right-hand corner. Sand on the surface indicates the mode of vibration.

Fig. 8 shows some fairly small turbine blades excited by a vibrator which is essentially a magnified radio loudspeaker. Instead of driving a paper cone, the "voice coil" here drives the turbine blade through a thin shaft or needle. The illustration on the front cover shows the same device exciting a bank of very large turbine blades, which is so complicated that a calculation of the natural frequencies is impracticable.

A very unusual case of vibration is shown in Fig. 9, which is a schematic view of a steam turbine that was found to vibrate as a solid body in the axial direction, i.e., a longitudinal sliding back and forth of the journals in their bearings. The frequency of this oscillation was the natural one determined by the total mass of the rotor and by the stiffness of the spring G. It was a case of instability caused by a pulsation in the steam pressure. Practically all of the high pressure steam entering the turbine at A flows to the left through the blading to the condenser H. A very small amount, however, leaks to the right past the "labyrinth" B into the chamber C and from there through E to the condenser. The labyrinth B is so constructed that a longitudinal displacement of the turbine rotor changes the cross section of the passage offered to the leaking steam, so that the rotor vibration is accompanied by a pulsating steam flow past B and consequently by a pulsating pressure in the chamber C. The phase relations in this case happened to be so as to make the system unstable: the pressure variations in C driving the rotor back and forth. An analysis of the mechanism by J. G. Baker showed that the insertion of a 2 cubic feet chamber in the easily accessible pipe E would change the phase relations sufficiently to render the system stable, which was verified by experiment.

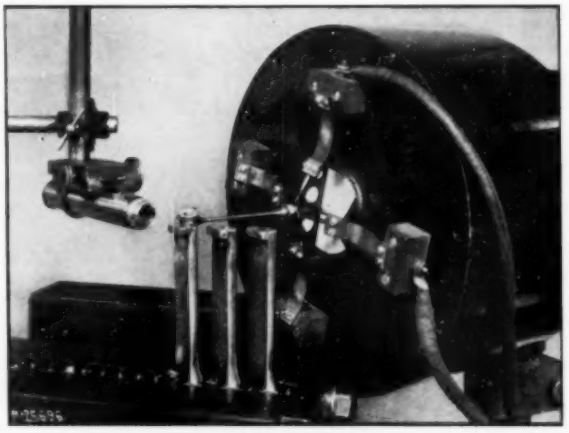

FIG. 8. Electrodynamic exciter for turbine blades (Westinghouse Electric).

A QUESTION of continuous importance is the balancing of rotating machines, i.e., the determination of the correction to be applied to a given rotor so as to make its principal axis of inertia coincide with the bearing axis. Many ingenious machines for this purpose have been

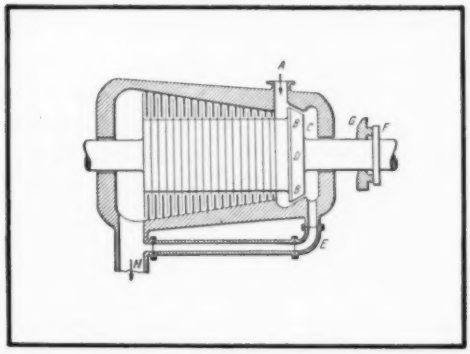

FIG. 9. Axial vibration of turbine caused by steam leakage.

invented, of which the two latest ones will be explained briefly.

Fig. 10 shows an apparatus, in which the rotor to be balanced is seen plainly in the center. On the right-hand end of its shaft, rotating with the rotor, is a small round "balancing head," shown in detail in Fig. 11. It contains two balls, which usually are clamped in whatever angular position they happen to be, but which can be released by finger pressure on the central knob. Then the balls are completely free in their circular race. The rotor in Fig. 10 is brought to a high speed by a belt drive. Its left-hand bearing is pivoted so that the (unbalanced) rotor oscillates slightly about that bearing, because the right-hand bearing is freely supported on springs, which do not impede its motion. While so running the

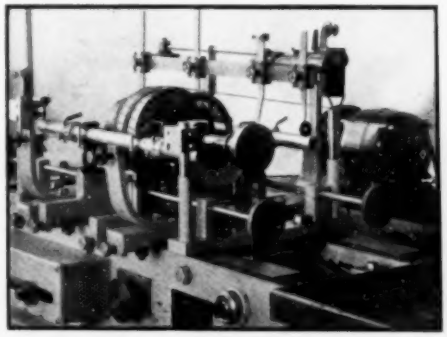

FIG. 10. Thearle balancing machine (General Electric Co.).

operator presses on the knob of the balancing head, thus releasing the two balls. The balls will then fly automatically to such angular positions as to counteract the existing unbalance in the rotor and to stop the vibration entirely.[7] Releasing the knob after a second or so, clamps the balls in this position, which can be read off immediately after stopping the rotor. This determines the unbalance of the rotor at its right-hand end, and a similar experiment is necessary to find the unbalance at the left-hand end.

The other machine is shown in Fig. 12. With it the necessity of pivoting first about one bearing and then about the other is avoided; the balancing of the right-hand end and the left-hand end is done simultaneously. The rotor to be balanced is seen in the foreground resting on two

FIG. 11. Thearle balancing head.

bearings, both supported on long, thin, vertical strip springs that do not impede forward and backward motion. The unbalanced rotor (driven by a belt) sets these two bearings vibrating and the vibration is directly transmitted to two loudspeaker elements shown in the figure. The two a.c. currents emitted by these loudspeakers contain all information (amplitude as well as phase angle) of the vibrations, i.e., they are equivalent to the knowledge of the two vibration vectors. The magnitudes and angular locations of the unbalance weights are two other, unknown, vectors, which are related to the vibration vectors through linear algebraic equations. To solve these two simultaneous vector equations is a fairly complicated undertaking, but that is done electrically with this machine. The loudspeaker currents are fed to a circuit that does the solving, so that the two magnitudes of unbalance weight can be read off immediately on milliammeters and the phase angles are revealed by stroboscopic light shining on the rotor and

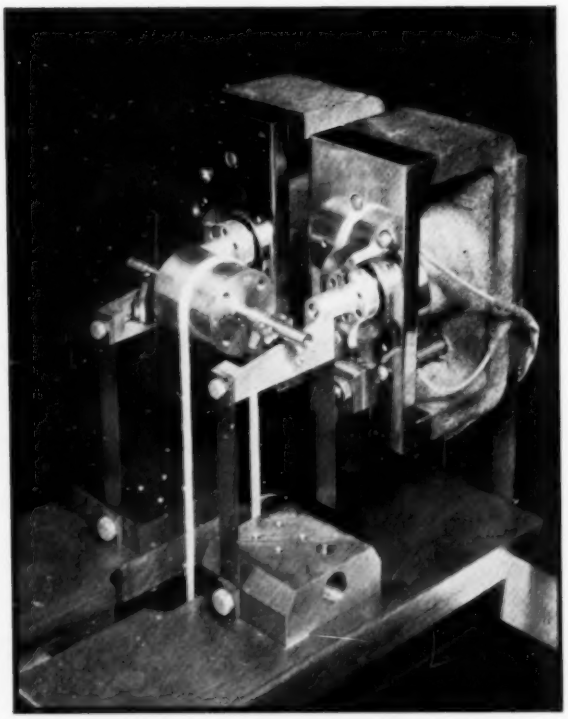

FIG. 12. Baker balancing machine (Westinghouse Electric).

setting it at rest apparently.[8] With this machine small rotors for domestic appliances can be balanced in 30 seconds apiece to a greater accuracy than is attainable with the older reversable-fulcrum-type balancing machines.

IT is not only in high speed rotating machinery that vibrations are of practical importance. They have caused great damage and worry in so static a structure as an electric transmission line. The fundamental cause for this phenomenon is clear from Fig. 13, showing the wake behind a solid cylinder past which water is flowing from right to left. Vortices or whirls are cast off periodically on alternate sides. Naturally such a flow must cause an alternating force in a direction perpendicular to that of the stream, and thus a transmission line against which a sidewise wind is blowing will be subjected to up and down alternating forces. If the frequency of this eddy shedding happens to coincide with one of the natural frequencies of the span, resonant vibrations build up which have broken many a line in fatigue. Since the eddy frequency depends on the wind velocity, which is uncontrollable, a knowledge of the natural frequencies of the span is useless to us, and for a remedy it is necessary to introduce dampers, which act on the same principle as explained with Fig. 2. A mass is suspended from the line by means of a spring which allows relative motion and a moderate amount of friction is provided between the mass and the line. A crude but adequate construction is known as the Stockbridge damper, shown in Fig. 14. The two masses of steel are attached to the line through a piece of stranded cable which acts as a spring and at the same time has sufficient friction between the individual wires of the cable to insure energy dissipation.

These are some of the problems that have been partly or completely solved during the last few years. However, there are many others in which the last word is very far from having been spoken. In the airplane industry the flutter of control surfaces is still giving trouble; the propeller blades still break off once in a while and the engine is still setting the whole plane in vibration. New cases of trouble in steam turbines and hydraulic turbines of a nature different from the ones we now understand are coming up almost every year. Though several practical devices exist for the suppression of the rolling motion of ships at sea, it is very desirable that they be superseded by simpler and more effective means. The very objectionable vibration in the stern of most of the larger ocean liners, caused by pressure variations in the water of propeller blade frequency, are beginning to be understood in

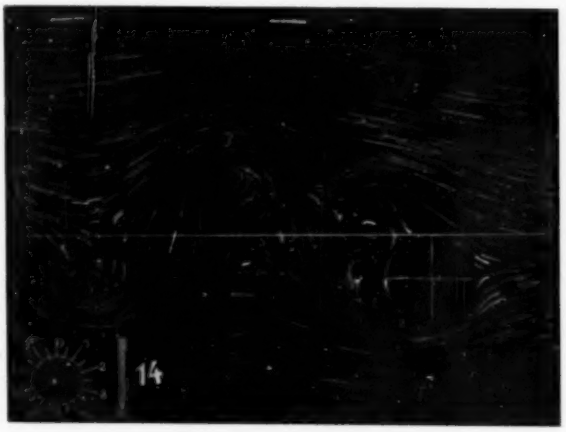

FIG. 13. Kármán trail of alternating eddies behind a cylinder.

detail and they should be eliminated. Grinding wheels for the accurate manufacture of rolls for the steel industry still vibrate more than a few millionths of an inch, which is not permissible. In the sewing machine industry, where new machines for special purposes are constantly being developed, inertia forces are still unbalanced in many cases, and this list could be continued almost indefinitely.

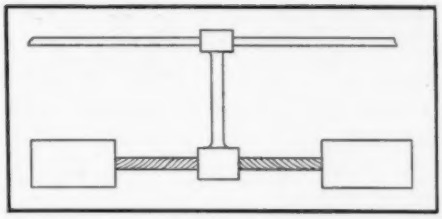

FIG. 14. Stockbridge damper for transmission lines.

In conclusion it may be stated that although the fundamental principles of mechanical vibration hardly transcend Newton's simple laws, which have been known for more than 250 years, the complete application of these principles to all sorts of machinery still provides large possibilities for the useful and creative work of the engineer and applied physicist.

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It is a rather sad commentary on a civilization which owes so much to science, and which could gain so much from further applications of science, that of the billions of dollars spent to relieve unemployment during the recent depression an almost negligible amount went to the support of any sort of scientific activity, in spite of the specific recommendations to this effect by numerous scientific groups and governmental bodies. It seems an economic crime that thousands of young and well trained scientists had to work on all sorts of "made-work" projects which made no use of their scientific ability when they would have been overjoyed at an opportunity to utilize their knowledge and acquire experience by working on the many scientific problems which are crying for solution, even on matters closely related to the governmental problems, as for example, agriculture, public health, housing and road building.

—ROBERT E. WILSON, "Looking Toward the Future of Invention."

Dr. Johnson

Modern Theory of Solids. I*

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Introduction

THIS series of articles is presented with a dual purpose: to emphasize the necessity for using quantum-mechanical concepts in discussing the properties of the solid state; and to review the progress of the modern theory based on these concepts.

Until recently, the various theories of different solids were conspicuously lacking in unity. To interpret the distinctive properties of the three solids copper, diamond, and rocksalt, for example, one had to begin with three widely different pictures of their internal constitution. And the attempts to interrelate these various pictures, or to fit them in with properties of the isolated atoms, had no very satisfactory success. In the present-day theory, atomic nuclei and electrons are taken as the prime entities, and the assumption is made that the same quantum laws always govern the behavior of these, whether they are isolated, are associated in small groups as atoms or molecules, or are piled together in vast numbers to form a macroscopic solid. A theory of the solid state based on this viewpoint is certain, obviously, to be a unified theory. Whether it is successful in explaining the properties of actual solids will depend on whether the fundamental quantum laws really are sufficient, on whether they are thoroughly known, and on whether one is skillful in deducing their large-scale consequences. Agreement between prediction and observation is the only decisive test. One of our aims, then, is to show how the quantum theory has succeeded in interpreting many of the observed properties of solids which the classical pictures left unexplained.

In addition we shall wish to survey, in nonmathematical fashion, the present status of development of the theory, to see where progress has been made and where large gaps in our understanding still exist.

This article is the first of a series of nonmathematical papers describing the modern theory of solids and its application to large scale properties of matter. The editor is particularly glad to be able to present these papers at this time because of the widespread interest of metallurgists in the fundamental developments which the physicist has been able to make in the last few years with regard to the nature of the solid state. Some of the properties to be considered in this series are cohesion, conductivity, photoelectric effect, thermionic emission, adsorption, fluorescence, photographic sensitivity and breaking strength.

—Editor.

* These articles are based on a talk given by one of us in a symposium on industrial physics at the Rochester meeting of the American Physical Society in June, 1936.

About half of the series will be devoted to a discussion of volume properties, such as cohesion, order of magnitude of electrical conductivity, etc., which are determined by the cooperative action of the principal atomic constituents. It is in this field that the theory has thus far been most successful.

Surface properties, which are involved in the photoelectric effect, thermionic emission, adsorption, etc., will be discussed in a part of the third article. This class of properties is not nearly so well understood as are the volume properties.

Structure sensitive properties, determined by the presence of impurities or small flaws in the lattice, have only begun to be the subject of theoretical investigation. They have tremendous technical importance, however—we mention fluorescence, photographic sensitivity and breaking strength of materials as fields where structure sensitive properties are primarily involved. In the final article we shall consider the current explanation of these properties.

I. Developments before Quantum Theory

1. Classification of solids

An acceptable general theory of solids has to explain a large body of facts accumulated during the past two hundred years by investigations necessarily empirical. These investigations have led to various divisions of solids into groups. Classification has been based on chemical constitution, on crystallographic symmetry, and on tensorial properties such as conductivity, elasticity, and dielectric behavior. Considering all these characteristics, one finds that solids can be grouped rather naturally into five prominent divisions:

(1) metals,
(2) ionic crystals,
(3) valence crystals,
(4) semiconductors,
(5) molecular crystals.

The metals are distinguished by high thermal and electrical conductivity. Most electropositive elements form solids in this group. Ionic crystals are marked by good ionic conductivity at high temperatures, strong infrared absorption spectra, and good cleavage. Practically all the salts, such as NaCl, MgO, etc., fall into this group. Valence crystals, such as diamond and carborundum, have poor electronic conductivity, great hardness, and poor cleavage. They are formed of the lighter elements in the middle columns of the periodic table. Semiconducting crystals are chiefly marked by a feeble electronic conductivity which increases with temperature. CuO, CuO_2 and ZnO are examples. Semiconductors usually resemble valence crystals in cleavage, hardness, and lattice structure, but do not always obey valence rules in combining. Molecular crystals, finally, are the class to which belong most solid organic compounds. They have low melting and boiling points, evaporate in general in the form of stable molecules, and behave, in short, as orderly aggregates of loosely bound molecules.

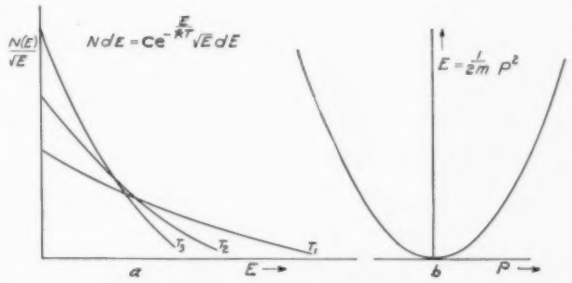

FIG. 1. (*a*) Energy distribution at several temperatures ($T_1 > T_2 > T_3 > 0°K$) of the free electron gas in a metal (Lorentz theory, Maxwell-Boltzmann statistics). The average energy is $(3/2)kT$, about 0.04 electron volts at room temperature. (*b*) Dependence of electronic energy E on momentum p.

There are, of course, many solids which lie intermediate among these five types. We shall use this classification, however, in arranging the discussion.

2. The Lorentz theory of metals

Before 1900, theoretical work on solids was mainly phenomenological. Various physical properties were interrelated by the use of thermodynamics and electromagnetic theory. This stage of development is summarized in the books of Drude,[1] Voigt,[2] and others. The few provisional attempts to interpret physical properties on the basis of a picture of the internal constitution of a solid[3] are interesting chiefly as historical landmarks.

The first work we shall treat is the theory of metallic conduction proposed by Lorentz in

1905.[4] He regarded the valence electrons in metals as particles of a perfect gas, free to move throughout the lattice, and distributed in energy according to Maxwell's law. The number-*versus*-energy function, and the variation of energy with momentum, are shown in Fig. 1. The average energy is $(3/2)\,kT$, where k is Boltzmann's constant and T is the absolute temperature. All the electrons have zero energy and zero velocity at $T=0$. When a potential difference is applied across a metal, the electron gas, on Lorentz' view, moves through the lattice under the force which the field exerts on the individual particles, and an electric current results. Resistance is interpreted as arising from elastic collisions of the electrons with the ions of the lattice. Lorentz showed that one could account for the magnitude of the resistance and for its temperature variation in the neighborhood of room temperature by assuming that all the valence electrons are free and that the mean distance traveled between collisions is approximately one lattice spacing.

This picture of an internal electron gas also fitted satisfactorily with the observations on electron emission from heated metals. If the potential energy of an electron outside the metal is chosen as zero, the total energy of an internal electron is $E=W+p^2/2m$, where W is the negative internal potential energy, and p is the magnitude of the momentum. At absolute zero, p is zero, and W is the work required to remove the electron from the metal—that is, W is the work function (Fig. 2). At higher temperatures, those electrons in the Maxwellian distribution which strike the surface, at $x=0$, with momentum components p_x so large that $W+p_x^2/2m$ is positive, can escape and be measured as a thermionic current. Richardson's[5] work showed that this simple picture is able to describe practically all the main features of thermionic emission.

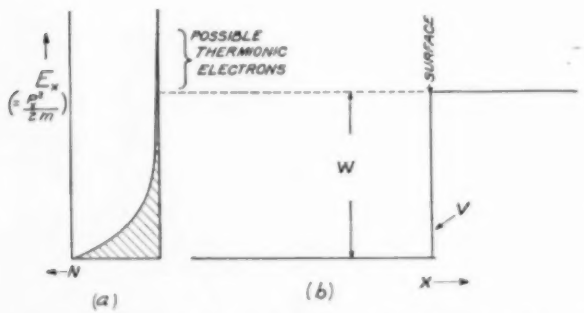

FIG. 2. Thermionic emission on the Lorentz theory. The few electrons in the Maxwellian distribution (*a*) reaching the surface with energy $E_x(E_x=p_x^2/2m)$ greater than the work function W (shown in (*b*)) can escape.

The theory faced a major difficulty, however, in assigning a heat capacity of $(3/2)\,k$ to each valence electron. Experimentally, the specific heats of most metals, like other solids, are within a few percent of the value $3R$ per mole, associated with the thermal motion of the lattice ions. So far as specific heat is concerned, the valence electrons in a metal behave not as if they were all free, but as if only about one in a hundred were free. To avoid this difficulty, one might assume arbitrarily that most of the valence electrons are bound to atoms while the remaining few are free to conduct current, but then the mean free path between collisions would have to be increased from about one lattice spacing to about a hundred, to account for the observed conductivity. In this dilemma, it had to be admitted that the theory, while basically plausible, was, on the whole, unsatisfactory. No promising new attack on the problem of metals was made until the advent of the quantum theory.

3. The Madelung ionic model

The next important development began with the attempt of Madelung[6] to calculate the binding energy of ionic crystals by treating them as lattice systems of positive and negative point charges, and determining the electrostatic energy of such configurations. Born, von Kármán, and many others used this ionic model during a long and fruitful period of computations.[7] They were able to interrelate many of the properties of ionic crystals in a semiquantitative way. Thus, the infrared absorption frequencies were connected with the elastic constants by considering the dependence of both on the assumed lattice forces. The Madelung model was most successful in handling the monovalent alkali halide crystals, and failed rather badly with divalent compounds such as MgO. The reason for this failure now appears to be, that the ion-lattice picture is not strictly correct in any case, and becomes less and less correct as the atoms involved approach the center of the periodic table.

4. Valence and van der Waals binding

The development of electron valence theories by Lewis,[8] Langmuir,[9] and others in the period following 1916 naturally aroused speculation on the electronic structure of valence crystals. These valence theories did not attempt a dynamic interpretation of the electronic bonds, but tried rather to correlate all cases through certain elementary concepts—principally, the "tendency of atoms to form closed groups"—which have empirical rather than mechanical justification. How these "atomic tendencies" are related to other characteristics of the atom, such as its spectrum, was not known. The quantum theory offers an insight into these tendencies, but it also indicates that the valence rules are not universal, and that each substance requires a special and unfortunately intricate consideration.

Cohesion in semiconductors was left unexplained before 1925. The Madelung model is certainly inapplicable, and furthermore these crystals do not always obey the valence rules.

Binding in molecular crystals was also not too thoroughly understood. It was generally believed[10] that the molecules are held together by the so-called van der Waals forces, which had originally been introduced to explain deviations in the behavior of real gases from that of a perfect gas. These forces were considered as due to the interactions of mutually-induced dipole or quadripole moments in neighboring molecules; no exact picture of the dynamical behavior of the valence electrons was forthcoming.

5. The problem of conductivity

It is worth emphasizing, before we leave this historical summary, that in all the classical pictures of nonconducting crystals the valence electrons were fixed to particular atoms or molecules or particular regions of space between particular neighboring atoms. Lack of conductivity was blamed on the inability of the electrons to move over long distances. On the other hand, the Lorentz theory, in spite of its defects, strongly supported the view that in metals some of the electrons are somehow free to roam throughout the lattice. Table I shows, for contrast, the conductivity, at room temperature, of several metals and of several typical nonconductors. It is evident that the vast gap in conductivity between metals and other solids must be due to some fundamental difference. If the valence electrons are free in metals and bound in nonconductors, it seems reasonable that metals should be strikingly different in other respects than in conductivity. Yet, in most of their other properties, conductors and nonconductors are very much alike. In particular, one finds both low and high melting points in all the various groups of solids (excepting molecular crystals), and one is thus led to suppose that the cohesive forces in conductors and in nonconductors arise from similar sources. The clarification of this problem of conductivity is one of the most notable advances of the modern theory.

TABLE I. *Resistivity for some metals and other solids (from International Critical Tables).*

Solid	T°C	ρ ohm—cm
Ag	20	1.6×10^{-6}
Al	20	2.8×10^{-6}
Be	20	10.1×10^{-6}
W	0	5×10^{-6}
B	0	1.8×10^{6}
C (diamond)	15	$\sim10^{14}$
SiO_2 (crystal)	20	$\sim10^{15}$
Mica		10^{9} to 10^{11}
Paraffin		10^{16} to 10^{19}

II. The Pauli-Sommerfeld Theory of Metals[11]

Very soon after the quantum laws of atomic systems were discovered, they were applied to the interpretation of the properties of solids. The first such application was by Pauli, who was able to explain the feeble paramagnetism of metals, which was previously mysterious.

Pauli supposed, as Lorentz had done, that all the valence electrons in metals are free. He assumed that the energy E of each valence electron is given by the classical relation

$$E - V_0 = p^2/2m, \tag{1}$$

where V_0 is the potential energy, taken to be constant throughout the metal, and p is the magnitude of the momentum. Then, in keeping with the quantum rules for atomic systems, he assumed that not all values of vector momentum $\mathbf{p}$ are allowed, but only those for which the components p_x, p_y and p_z satisfy the relations

$$p_x = \frac{n_x h}{L_x}, \qquad p_y = \frac{n_y h}{L_y}, \qquad p_z = \frac{n_z h}{L_z}.$$

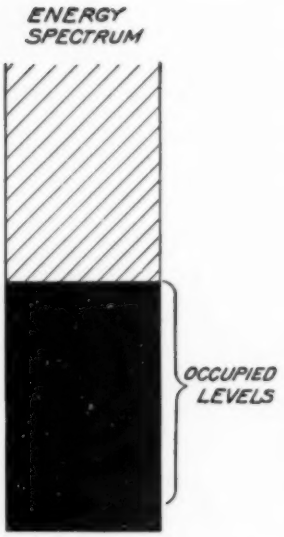

FIG. 3. Energy distribution at $T = 0$, when the free electrons are subject to the exclusion principle. The filled levels extend over several electron volts.

Here n_x, n_y and n_z are arbitrary integers, L_x, L_y and L_z are the lengths of the edges of the crystal sample along the x, y and z directions, respectively, and h is Planck's constant divided by 2π. This means that the allowed energy values for electrons in a finite crystal are not continuous as in the Lorentz theory, but discrete. However, the energy difference between adjacent energy levels turns out to be so extremely small for a crystal of ordinary size (1 cm on edge, say) that the energy spectrum could be regarded as continuous, except for another quantum rule, the Pauli exclusion principle.

The exclusion principle says that the lowest state ($n_x=n_y=n_z=0$) can contain at most two electrons, one with its spin vector in, say, the positive z direction, and the other with its spin vector oppositely directed. The same statement is made for each of the levels corresponding to other values of n_x, n_y and n_z; no more than two electrons in the entire crystal can have the same vector momentum and the same energy. At absolute zero of temperature, in the absence of external fields, only the lowest levels are filled, and we have for a metal with one valence electron per atom the energy-level diagram of Fig. 3: a filled band of levels is immediately overlain with an empty quasi-continuum of levels. The average energy of the electrons at $T=0$ is not zero, as it was in the Lorentz theory, but is several electron volts, and the average momentum is correspondingly large. In the filled band, there are two electrons for each set of n's. The net magnetic moment of the crystal is zero, since each electron with spin in one direction is balanced by another with spin in the opposite direction.

In the presence of an external magnetic field of intensity H, the energy of a free electron is no longer given simply by (1), but is changed by a term $\pm(eh/2mc)H$, where $eh/2mc$ is the magnetic moment of the spinning electron, and the sign depends on whether the moment is antiparallel or parallel to the field H. The energy level associated with a given momentum thus splits into two levels; the one, corresponding to a moment parallel to the field, decreases by $(eh/2mc)H$; and the other, corresponding to antiparallel moment, increases by the same amount (Fig. 4). The distribution giving minimum total energy is then no longer than that in which equal numbers of electrons have the two directions of spin. The number having moment parallel to the field increases, the number with antiparallel moment decreases, and the crystal as a whole acquires a magnetic moment.

The susceptibility computed from this model agrees in order of magnitude (10^{-6} c.g.s.) with the measured susceptibilities of the simpler metals. The deviations can be attributed largely to electron interactions which the picture neglects.

The success of this first application of quantum

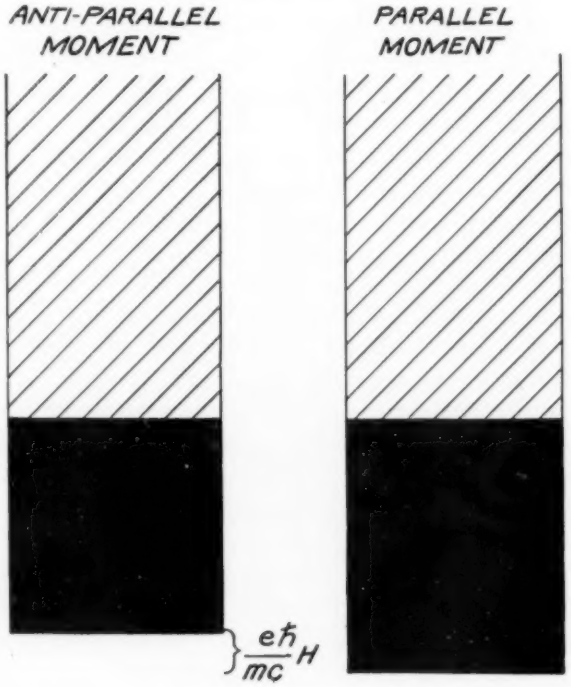

FIG. 4. Displacement (greatly exaggerated) of the energy states of different spin in a magnetic field. In equilibrium both bands are filled to the same height, so there are more electrons with magnetic moment parallel to the field than anti-parallel.

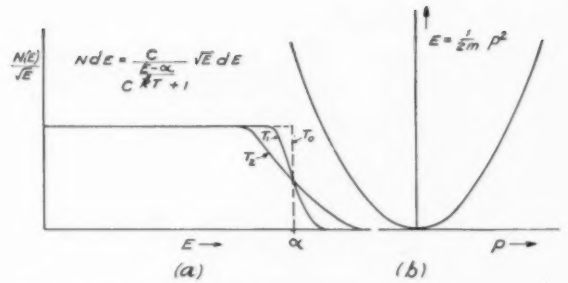

FIG. 5. (*a*) Energy distribution at several temperatures ($T_2 > T_1 > T_0 = 0°K$) of a free electron gas subject to the exclusion principle (Pauli-Sommerfeld theory, Fermi-Dirac statistics). The energy α is several electron volts. (*b*) Dependence of electronic energy on momentum. This is the same as in the Lorentz theory (see Fig. 1).

theory led Sommerfeld to reconsider the Lorentz theory of metallic conduction, using the new concepts. When the particles of a gas are subject to the exclusion principle, the distribution-in-energy varies with temperature according to the Fermi-Dirac function, rather than the Maxwellian. The difference between the two is marked at low temperatures or when the density of the gas is high, and the valence-electron gas in a metal is so dense (2.56×10^{22} particles/cc for Na, for example) that the Maxwell law is quite inapplicable. The more correct Fermi-Dirac distribution is shown in Fig. 5, for several temperatures. Increase of temperature affects the $T=0$ distribution only in the neighborhood of higher energies, smoothing it out to an exponential decrease. Most of the free electrons have energies corresponding to the flat part of the curves. Though these electrons are moving freely throughout the lattice, they behave in two important respects as if they were bound.

First, as is evident from Fig. 5, they do not contribute to the specific heat. When the temperature of the metal is raised, only those electrons which are already near the top of the energy distribution can gain still more energy. By the exclusion principle, an electron in one of the lower levels cannot increase its energy slightly, since the level to which it would have to move is already occupied. Thus, the chief difficulty of the Lorentz theory is automatically avoided.

Second, if an e.m.f. is applied across the metal, the only electrons that can change their momenta—that can contribute to a current in the direction of the field—are those in the upper filled levels of the energy spectrum, near the vacant states. As Bloch showed, the probability that an electron will change its energy by more than a small fraction of a volt is negligible, unless the field is extremely large. The electrons in the lower levels are unaffected by an external field, and carry no net current, since for every one with momentum $\mathbf{p}$ there is another with momentum $-\mathbf{p}$.

Sommerfeld's equation for conductivity resembles that of Lorentz very closely, with the few electrons in states bordering the unoccupied levels playing the same role as the free electrons in the Lorentz theory.

In the Sommerfeld equation, as in that of Lorentz, appears a parameter having the significance of a mean free path. This distance, to fit the observed conductivities, must be about a hundred lattice spacings, since only about one percent of the free electrons assist in conduction. Just as in the Lorentz theory, such a long mean free path had to be assumed arbitrarily. However, when the wave properties of electrons, to be described presently, are taken into account, a detailed consideration shows that a free path of this order of magnitude is to be expected naturally. It turns out, at variance with Lorentz' idea, that the conducting electrons do not make collisions with the lattice ions so long as the ions are stationary. Bloch found that the electrons suffer *inelastic* collisions with the ions when these undergo thermal vibrations. The free path between such collisions is not directly connected with the interatomic distance, and is strongly temperature-dependent at low temperatures. The temperature dependence computed by Bloch is shown in Fig. 6. It has the proper order of magnitude at room temperature, though the low temperature treatment is not entirely satisfactory. Later calculations using the same approach have improved this aspect.

As for thermionic phenomena, the Pauli-Sommerfeld picture fitted with the observations just as completely as did the Lorentz theory. The Fermi-Dirac distribution and the Maxwellian distribution both have the same exponential shape at high energies, and it is only the high energy electrons that are emitted thermionically.

The Pauli-Sommerfeld theory thus offered a self-consistent interpretation of most of the general properties of metals. The major question,

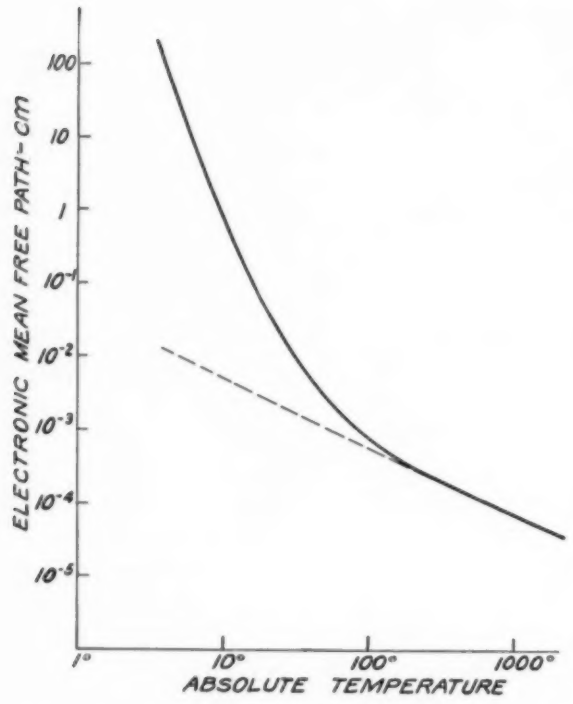

FIG. 6. Temperature dependence of mean free path of electrons in a metal, as calculated by Bloch. The dotted line shows the dependence predicted by the Lorentz theory.

what is the essential difference between metals and nonconductors, remained approximately as obscure as it had been in 1925.

III. The Zone Theory of Solids[12]

1. Electrons in a periodic field

During the past few years a general theory of the solid state has been developing, which offers an explanation of the difference between conductors and nonconductors, and which is having considerable success in interpreting many of the other properties of solids. This modern theory differs from the Pauli-Sommerfeld theory in including two salient facts: the wave nature of the electron, and the periodic potential distribution in the solid lattice.

The wave mechanics picture may be summarized in a few words as follows: With every particle moving with momentum p is associated a wave of wave-length $\lambda = h/p$. The square of the amplitude of this wave at any point in space is proportional to the probability of finding the particle there. For an electron of mass m, moving with constant total energy E in a potential field $V(x, y, z)$, the wave function $\psi(x, y, z)$ is given by the Schrödinger equation

$$\nabla^2\psi + (8\pi^2 m/h^2)(E - V)\psi = 0.$$

The reader is undoubtedly acquainted with the success of the picture in interpreting atomic spectra—quantization rules, selection rules, and the uncertainty principle fall naturally out of it—and with the electron-diffraction experiments of Davisson and Germer, Thomson, and others, which further demonstrate its correctness.

We shall need to discuss, first, the spectrum of possible energies of wave electrons moving in a periodic potential field, and, second, how these energy levels are populated.

The solutions of the Schrödinger equation for an electron in a solid are most conveniently described in terms of the *wave number vector*, $\boldsymbol{\sigma}$ (in magnitude, $\sigma = 1/\lambda$). This quantity has the dimensions of momentum divided by action. If the electron moves with constant total energy E in a constant potential field V_0, such as the Lorentz and Pauli-Sommerfeld theories pictured for the interior of a metal, then

$$E - V_0 = p^2/2m = h^2/2m\lambda^2 = h^2\sigma^2/2m,$$

and

$$\mathbf{p} = h\boldsymbol{\sigma}.$$

That is, the energy E varies parabolically with p and therefore parabolically with σ (Fig. 7*a*). In an actual solid lattice, however, the potential field in which a given electron moves is not uniform, but is a complicated function of position. It depends not only on where the atomic nuclei are located, but also on how all the other electrons move. The potential distribution, the

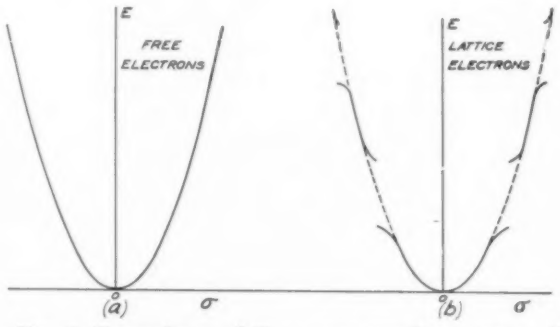

FIG. 7. Dependence of E on wave number vector σ, for (*a*) an electron in a uniform potential field, and (*b*) an electron in a simple one-dimensional periodic field.

wave functions ψ of the individual electrons, and the charge distribution must be calculated all together, by methods of approximation which we shall not go into. It is fairly obvious, however, that the average of the potential and of the electronic charge distribution must both be periodic, with the symmetry of the lattice. Strutt, Morse, Peierls, Brillouin[13] and others found that in such a periodic potential field the energy E of an electron is not proportional to σ^2, as it is in a uniform field, but depends on the configuration of the field and on the direction of $\boldsymbol{\sigma}$ as well as on its magnitude. For a given direction of $\boldsymbol{\sigma}$, $E(\sigma)$ generally has such a form as is shown in Fig. 7*b*. At certain values of σ, the energy E has discontinuities—certain ranges of energy are forbidden to the electron, if its wave number vector has that direction. For a different direction of the vector $\boldsymbol{\sigma}$, these discontinuities will, in general, leave different energy ranges forbidden. Unless a forbidden range for one direction of $\boldsymbol{\sigma}$ is completely covered by allowed ranges for other directions, there will be a gap in the energy spectrum, which no electron, whatever its momentum, can enter. To find out whether such overlapping of allowed ranges does occur, it is necessary to examine $E(\boldsymbol{\sigma})$ for each of the possible directions of $\boldsymbol{\sigma}$. Fortunately, the question can usually be answered by calculating $E(\boldsymbol{\sigma})$ for prominent lattice directions, since the energy function may be expected to have its extreme behavior in these directions. Fig. 8 shows the two possibilities. In Fig. 8*a*, the allowed ranges for the three directions of $\boldsymbol{\sigma}$ overlap, so that no energy value is absolutely forbidden. In Fig. 8*b*, on the other hand, the allowed ranges do not completely overlap, and a forbidden range exists.

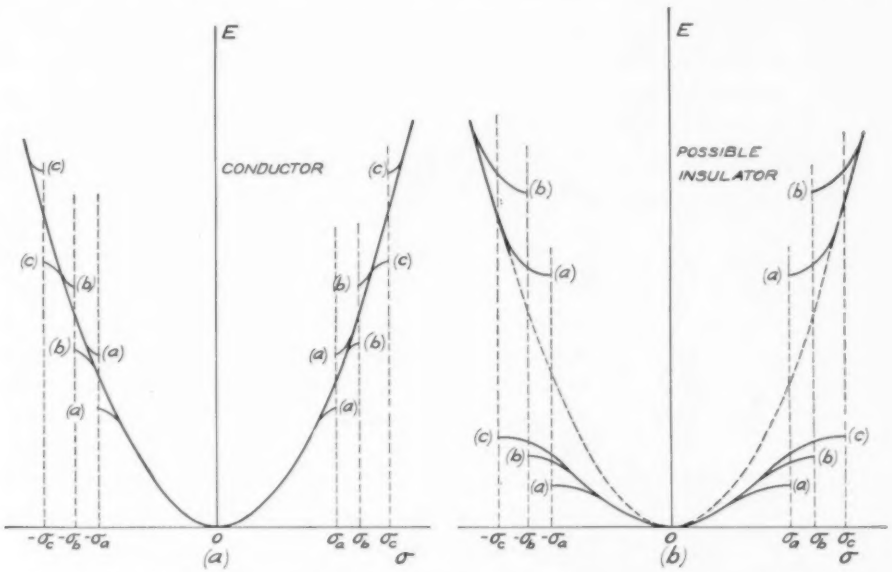

FIG. 8. (*a*) $E(\sigma)$ for three typical directions in a cubic lattice (the (111), (110) and (100) directions, for example). Note that the gaps occur at different points $\pm\sigma_a$, $\pm\sigma_b$, $\pm\sigma_c$, and that the allowed ranges overlap, leaving no completely forbidden range of energy. (*b*) The gaps here are so wide that complete overlapping does not occur, and a certain range of energy is left unallowed.

Fig. 9 shows some equal-energy contours for an electron moving in a simple potential field periodic in two dimensions. The discontinuities in the $E(\sigma_x, \sigma_y)$ function occur at the boundaries of regular polygons centered about $\sigma_x = \sigma_y = 0$. For a three-dimensional periodic field the corresponding equal-energy contours are complicated surfaces in a $\boldsymbol{\sigma}$ space, with discontinuities which occur at boundaries of regular polyhedra centered about the origin.

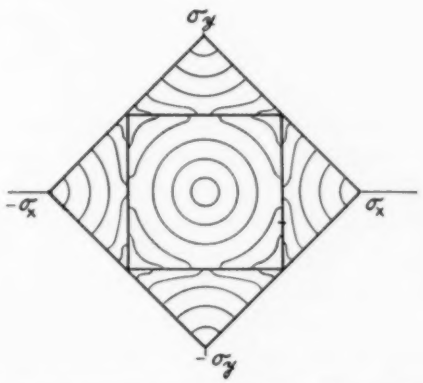

FIG. 9. Equal energy contours (drawn light) for electrons in a simple two-dimensional periodic field. The discontinuities in energy occur at the boundaries of regular polygons (two squares are shown). For a three-dimensional periodic field the discontinuities occur at the surfaces of regular polyhedra.

In the Pauli-Sommerfeld theory, the energy spectrum was practically a continuum of discrete levels—discrete, because the momentum components p_x, p_y and p_z were quantized. In the new picture, the discrete levels still appear—in fact, the quantization now comes in automatically as a consequence of the wave nature of the electron. But it turns out that the energy levels are no longer so uniformly spaced. If we have a crystal block containing N unit cells, the

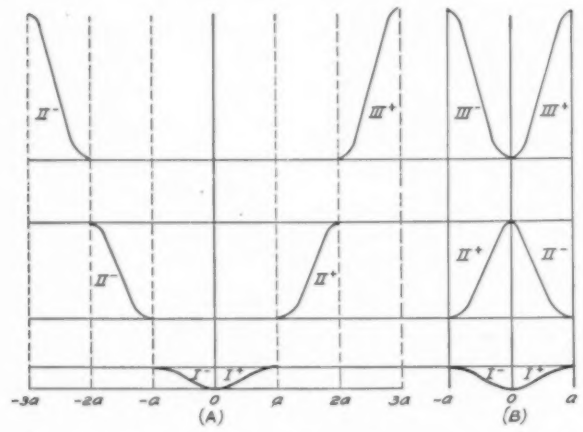

FIG. 10. Illustrates how the continuous ranges of the $E(\sigma)$ plot on the extended zone scheme (A) are displaced horizontally to form the more compact reduced zone scheme (B).

energy spectrum of the electrons has to be divided into sets of N levels. Within each such set, the N levels are always so closely spaced that, for all practical purposes, they form a continuous band, as did the levels in the Pauli-Sommerfeld picture. We shall refer to such a band of N levels as a zone. In the one-dimensional case (Fig. 7*b*), each continuous range of energy between the discontinuities in the $E(\sigma)$ function contains the N energy levels comprised in one zone, and a similar correspondence holds in three dimensions. The distribution-in-energy of the discrete levels thus depends directly on the way in which the energy varies with wave number vector. When the forbidden ranges of energy, for one direction of $\boldsymbol{\sigma}$, are covered by allowed ranges for other directions, the zones will overlap. When they are not so covered, the zones will be separated by energy-gaps in which no allowed levels exist.

We next ask which levels in this zone type of energy spectrum are occupied by electrons. Each level in a zone is doubly degenerate, in the sense that it can contain two electrons, with spins oppositely directed. The exclusion principle still restricts the population of a given level to no more than these two electrons, so the electrons are distributed among the levels according to the Fermi-Dirac function, as in the Sommerfeld model. At $T=0$ the lowest levels are all doubly occupied, and all the levels above these are completely empty. At higher temperatures a few of these higher levels can be occupied by electrons thermally excited from levels immediately lower.

It is to be noted that whenever the forbidden ranges in energy are narrow (Fig. 8*a*) the $E(\sigma)$ curve, except in the immediate vicinity of a discontinuity, is closely parabolic, like that of a free electron (i.e., an electron in a uniform field). We may consequently expect the electron to move largely as if the potential field were uniform instead of periodic. Conversely, we may expect less similarity to free electron behavior when the allowed zones are narrow and the unallowed are wide—the situation sketched in Fig. 8*b* is an example.

There is another way of plotting the relation between E and $\boldsymbol{\sigma}$ which does not emphasize the deviations from the free electron relation, but has compensating advantages. We shall illustrate it by a one-dimensional example and then state the result of generalization to the actual three-dimensional lattice. The $E(\sigma)$ plot for a one-dimensional periodic potential field is shown in Fig. 10*a*. The discontinuities occur at $\sigma=\pm a$, $\pm 2a$, $\pm 3a$, $\cdots \pm na$. Now, if we move the curve between a and $2a$ horizontally back to the range $-a>\sigma>0$, move that between $-2a$ and $-a$ horizontally over to the range $0>\sigma>a$, and do the same for all the other continuous sections between discontinuities, we finally get the scheme of Fig. 10*b*. E is then a many valued function in the range $-a>\sigma>a$, and *each branch is continuous*. Each branch corresponds to one zone, and we may number them consecutively as zone 1, zone 2, etc. We shall refer to this sort of $E(\sigma)$ plot as *the reduced zone scheme*, in contrast to *the extended zone scheme* discussed previously.

Exactly the same procedure may be carried through in the two- and three-dimensional cases. In two dimensions, the discontinuities occur at

the boundaries of regular polygons centered about $\sigma=0$. A simple case is shown in Fig. 9. The area between any one of these polygons and the next largest is always equal to the area of the first, smallest polygon, so that the energy function in each such region may be mapped in this first polygon. E will then be a many valued function in this first polygon, and a consistent plan of mapping gives continuous energy surfaces, like the continuous curves in the one-dimensional case. In three dimensions, the discontinuities are at boundaries of regular polyhedra centered about $\sigma=0$; the volume between successive polyhedra is equal to the volume of the first; and all the energy functions in these regions may be mapped in this first polyhedron as a set of continuous functions $E_1(\sigma)$, $E_2(\sigma)$, etc., with σ extending over this polyhedron alone. When this is done, curves of the type shown in Fig. 8 may be replaced by the system of Fig. 11.

It turns out that two neighboring energy functions are generally not equal at the same value of σ—that is, they generally do not cross. In particular *degenerate cases*, however, they may be equal at $\sigma=0$, or for all values of σ in a given direction, such as along a symmetry axis of the lattice. In nondegenerate cases they are often equal at different values of σ; it is this kind of equality that corresponds to the overlapping of zones in the energy spectrum.

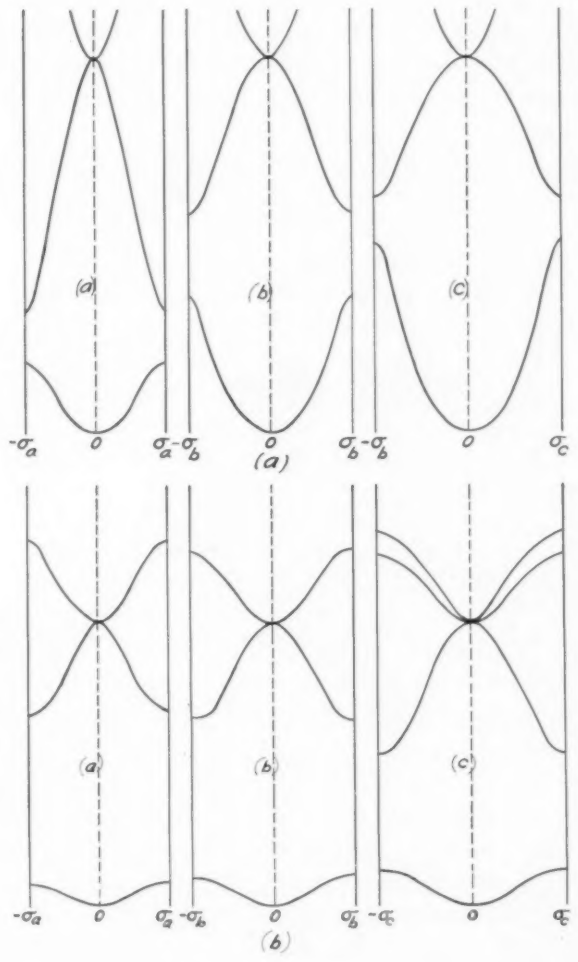

FIG. 11 (*a*) and (*b*). Illustrates the reduced zone plots corresponding, respectively, to the extended zone plots of Fig. 8 (*a*) and (*b*). A larger energy range is included, to show the characteristic behavior of upper zones. In the lower example, the second band contains three zones. Two of these are degenerate in the *a* and *b* directions, so only two curves are shown. In the *c* direction this degeneracy is not present. This case is similar to that of LiF.

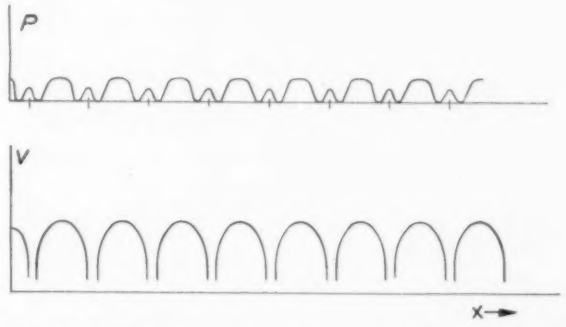

FIG. 12. The lower curve symbolizes the periodic potential field in a lattice; the upper curve, the corresponding periodic probability distribution of electronic charge.

The reduced zone scheme has several advantages, notably compactness. This advantage becomes apparent if four or five zones are to be plotted. Also, the selection rules for absorption of light are expressed quite simply in terms of this scheme.

We emphasize here that we are viewing a solid as a large molecule (containing, for example, 8.5×10^{22} atoms if the sample is a copper crystal 1 cm in edge). Each valence electron is in a definite energy state, just as are the electrons in an isolated single atom, and each valence electron is free to wander throughout the solid just as the electrons in the isolated atom are free to move in the potential field of the nucleus and the other electrons. The probability of finding an electron at a given point in the lattice depends on the configuration of the potential field, and has the periodicity of this field (see Fig. 12).

Bloch, in a discussion of metals, was the first to use this mode of description.

We have been thinking of each electron as moving in the *averaged* potential field of the nuclei and all the other electrons, and the zone structure of the energy spectrum is based on this notion. A closer examination shows that the picture is not strictly correct. Classically, each electron has a space around it in which another electron is not likely to be found, because of the mutual electrostatic repulsion. The exclusion principle, also, forbids two electrons to approach each other too closely in space; the magnitude of this virtual repulsion depends on how nearly the two electrons have the same momenta, and on whether their spins are parallel or anti-parallel. Both these electronic interactions have analogs in the wave mechanical picture, and both are neglected in the zone theory as we have described it. It turns out, however, that for many purposes the zone picture is a fairly good approximation for *all valence electrons* both in metals and in nonconductors (even, for example, for the eight valence electrons in NaCl). We shall use it with its limitations in mind. Electrons in the closed atomic shells, which are concerned in x-ray spectra, are, on any picture of the solid, actually bound to particular atoms. We shall generally leave these electrons out of the discussion.

2. Conductivity in the zone theory

Since the valence electrons are free to move through the lattice, in nonconductors as well as in metals, we must look to the zone structure of the electronic energy levels for an explanation of the difference between conductors and nonconductors. We shall examine several cases in detail.

Suppose, first, that the zones do not overlap, so that there is one lowest zone containing N levels, which is separated by several volts from the next lowest zone (Fig. 13*a*). Suppose that the solid under consideration contains one valence electron per unit cell (e.g., an alkali metal). The lowest zone will be half-filled (Fig. 13*b*). Electrons will be moving in all directions through the lattice, and there will be no net electric current. If a field is applied, some of the electrons of highest energy will be transferred to the higher states lying closely above, the statistical equilibrium will be disturbed, and a current will flow. This is identically the case discussed by Sommerfeld. It is thus evident that the solids of the monovalent alkalis, with one atom in the unit cell, must be metals.

Suppose, instead, that each unit cell of the solid under consideration contains two valence electrons, the zones still being widely separated. Then, the first zone will be completely filled (Fig. 13*c*). If an electric field of ordinary magnitude is applied, the lowest unfilled levels are now so far above the uppermost filled levels that practically no electrons can be raised to them. The statistical equilibrium is practically undisturbed, and practically no current flows. Simple computations show that a field of about 10^6 volts/cm would be required to cause a measurable current. Though the valence electrons are free to move throughout the lattice, the substance is a good insulator. Actually, some substances with two valence electrons per unit cell, such as Ca, are good conductors. We infer that the zones here are not separated but overlap (Fig. 13*d*). The computations show that the zones actually do overlap in both the alkaline earth metals and the alkali metals. It is clear that the alkalis would be conductors even if the zones were separated, while the overlapping is

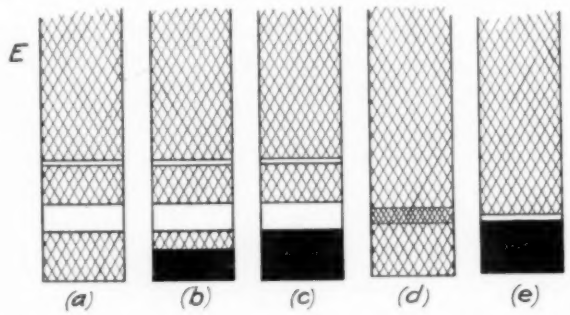

FIG. 13. (*a*) Typical energy spectrum for valence electrons in a crystal. The shaded regions are allowed, the clear regions are prohibited. (*b*) The lowest band is half-filled, the crystal having an odd number of valence electrons per unit cell. The crystal is a good conductor. (*c*) The lowest band is completely filled (even number of valence electrons per unit cell) and is distant from the next allowed band. The crystal is a good insulator. (*d*) The allowed bands overlap. The crystal is a good conductor, whether the unit cell contains an even or an odd number of valence electrons. (*e*) The lowest band is filled, but the gap above it is narrow. Electrons thermally excited to the upper band, and the "holes" they leave in the lowest band, allow the crystal a feeble electronic conductivity at ordinary temperatures.

essential to the conductivity of the alkali earth metals. Where the filled and unfilled zones overlap, the substance will be a conductor, whether the unit cell contains an even or an odd number of valence electrons.

All valence and ionic crystals, on the other hand, have an even number of valence electrons in the unit cell. Since they are good insulators, we conclude that the occupied zones are separated from the unoccupied. To avoid overlapping, the allowed zones must be narrow; one finds, in fact, that it is just this narrowness of the zones that characterizes the nonconductors, as contrasted with the metals.

If the energy gap is small (Fig. 13*e*), electrons from a completely filled lower zone may be thermally excited to the upper zone, so that the solid will acquire a measurable electronic conductivity, which will increase with temperature.

3. Zones and atomic energy levels

Some interesting correspondences exist between the electronic states in the isolated atoms and the zones in the solid. In many of the simpler solids each zone can be associated with a particular atomic state by tracing the behavior of the zones as the interatomic spacing is continually increased while the symmetry of the lattice is preserved. The zones become narrower and narrower and finally are reduced to the atomic levels.

In the isolated atoms, the energy levels are completely narrow, and particular valence electrons are certain to be found in "orbits" about the particular nuclei. Analogously, when the energy zones in the solid are narrow, and when each zone can be traced definitely back to a parent atomic level, any particular valence electron is very likely to be found in some sort of quasiatomic orbit about *one or another* of the lattice nuclei. To this extent the modern theory agrees, in certain cases, such as the alkali halides, with the classical concept that the "atomic character" of the constituent atoms is less disturbed in the formation of an insulating solid than in the formation of a metal. In no case does it support the classical view that particular valence electrons in insulators are bound to particular atoms.

If the formation of the lattice has a pronounced effect on the electronic orbits, as it does in practically all metals, the zones may become very broad and may overlap in intricate fashion: the association with atomic states becomes more symbolic than actual. When the overlapping brings filled and unfilled levels into adjoining positions, the solid is a conductor. Such overlapping becomes very probable when the atomic states are very dense, as they are in almost all atoms of large atomic number.

In the alkali halides, it proves simplest to trace the zones back to energy states of the isolated ions. With this convention, it is possible to refer unambiguously to the Cl^- $3s$ and $3p$ zones, for example. The zones are narrow at the equilibrium spacing, and the atomic (ionic) character of the electronic orbits is largely preserved.

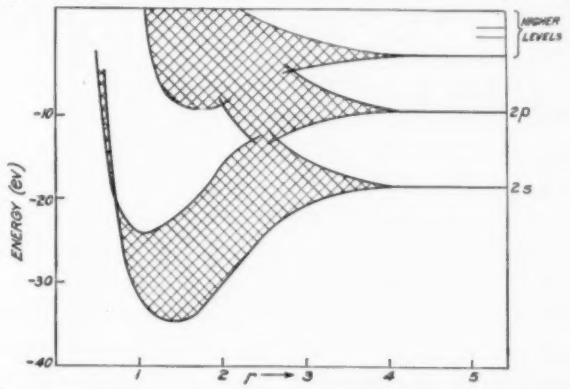

FIG. 14. Semi-quantitative representation of the zone scheme of diamond, varying with lattice spacing.

When the atomic states are well separated, as in carbon, for example, it may happen that as the lattice spacing is decreased the zones meet, intermingle, and then separate into groups (Fig. 14), the components of each group coming from various atomic levels.[15] Such a solid cannot conduct if the filled levels extend to the edge of a forbidden region. We shall discuss cases such as these more fully in the next article.

In building up molecular crystals we may proceed from either of two beginnings. Observing the electronic energy states of the constituent molecules as the lattice spacing is decreased, we expect to find that at the observed intermolecular spacing these have not broadened enough to overlap. If we start with the energy states of the atoms, we expect, especially if carbon is present,

that the zones will have overlapped intricately and at the actual interatomic spacing will have separated into groups. The final distribution of zones will, of course, be the same, whichever starting point we choose.

4. Binding energy

The cohesive energy of a solid—the work required to dissociate one gram-molecule into its constituent atoms— is the algebraic sum of the energies of the electrons in the occupied states and the energy of nuclear repulsion. The electrons as a group have lower energy in the solid than in the isolated atoms, and this term therefore favors binding. The nuclear repulsion becomes larger, of course, as the spacing between atoms is decreased, so this term opposes binding. The sum is small, compared with the two individual terms; one must therefore be careful in computing these terms if the sum is to be at all reliable. The nuclear repulsion energy is relatively easy to calculate, as is the energy of the electrons in the closed shells, which are not greatly affected by the formation of the solid. It is the valence electrons, moving in complicated orbits through the lattice, that give trouble. If we have the energy spectrum of these (the zone diagram) at the actual lattice spacing, then to a first approximation the total energy of all the valence electrons is the sum of the energies of the individual electrons in the occupied levels. But this must always be corrected to take into account the detailed electronic interactions which, as we have pointed out, the zone picture neglects. The correction terms which arise from the virtual repulsion introduced by the exclusion principle are usually called *exchange* terms. Those which arise from the fact that two electrons are unlikely to be close together, because of their mutual electrostatic repulsion, are called *correlation* terms. Both terms generally favor binding, and both are of the same order of magnitude as the net binding energy, so they have to be included to get a correct value. When they are neglected, the computed cohesive energy usually has the wrong sign, so that the solid appears to be highly unstable at the zero of temperature—this in face of the fact that all substances form solids at temperatures sufficiently low.

We emphasize that there is nothing mysterious about these exchange and correlation energies. The fundamental cohesive energy of a solid comes from the familiar Coulomb attraction[15] between positive nuclei and electrons. From this must be subtracted the Coulomb energy of ion-ion repulsion, and that part of the Coulomb energy of electron-electron repulsion and of the kinetic energy of the electrons which is not retained in the isolated atoms. The exchange and correlation energies are important because, though sometimes relatively small, they hold the balance between the other larger terms. Actually, they are corrections to the electron-electron repulsion as it is included in the simple zone picture, and they have to be made because the zone picture in itself does not completely describe this repulsion.

Can the zone picture be modified so as to include completely these detailed electronic interactions? Unfortunately, it cannot, without losing its most useful feature. When we assign an electron to a particular energy level in the zone system, we effectively assign it to a definite orbit in the solid, and assume that it continues in this orbit unless its course is altered by some external field. If we treat the detailed electronic interactions, the "electron collisions," it is no longer possible to assign any electron to a definite permanent orbit. In place of energy levels for individual electrons, we must say that the n electrons as a group occupy n levels. The same problem arises, of course, in computing the electronic structure of complicated atoms. The approximation we are using in the zone theory, that each electron moves in the average field of the nuclei and the other electrons, is called Hartree's. The zone picture is useful and adequate when certain general properties of the solid are to be discussed, but we must not look too closely into the precise motion of individual electrons.

It should be recognized that binding energies are computed in the same way, at least in principle, for all solids. The wide range of observed values among different solids arises from detailed differences in the distribution of energy states and in their occupation by electrons. We shall point out cases where certain peculiarities in the distribution of electronic levels may ac-

count for observed peculiarities in the cohesive properties of the crystal.

We shall begin the next article with a discussion of the fine solid types, and shall see how the zone picture accounts for many of their distinguishing characteristics.

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[14] We have in mind the ordinary "chemical forces." In nuclear physics it is necessary to include non-Coulomb forces between elementary constituents.
[15] Kimball, J. Chem. Phys. **3**, 560 (1935).

The story of the electron illustrates in vivid manner a number of characteristics of scientific work, some of which I shall simply enumerate: (1) progress comes by spurts of advance as some big new idea opens up new territory, alternating with periods of consolidation; (2) progress comes not by revolution or discarding of past knowledge and experience, but is built upon past experience and is its natural extension once the vision from new vantage points is secured; (3) there is nothing so practical in its values as accurate knowledge, and the pursuit of such knowledge has been most successful when not fettered with the initial demand that it be directed toward practical ends.

—K. T. Compton, Address of the Retiring President to the American Association for the Advancement of Science, December 28, 1936.

Advisory Council on Applied Physics

of the

American Institute of Physics

Report of Meeting
New York, October 28, 1936

I

THE meeting was called to order by Chairman Grondahl who proceeded to call the roll. The following were present:

Members

CARL L. BAUSCH, *Bausch and Lomb Optical Company*
LYMAN J. BRIGGS, *Director, National Bureau of Standards*
DETLEV W. BRONK, *Johnson Foundation, University Hospital, Philadelphia*
O. E. BUCKLEY, *Executive Vice President, Bell Telephone Laboratories*
WM. W. BUFFUM, *Treasurer and General Manager, The Chemical Foundation*
GEORGE O. CURME, JR., *Vice President, Carbide & Carbon Chemicals Corporation*
WHEELER P. DAVEY, *Research Professor of Physics and Chemistry, Pennsylvania State College*
W. F. DAVIDSON, *Director of Research, Brooklyn Edison Company*
MARION EPPLEY, *President, Eppley Laboratory, Inc.*
HARVEY FLETCHER, *Physical Research Director, Bell Telephone Laboratories*
PAUL D. FOOTE, *Executive Vice President, Gulf Research & Development Company*
L. O. GRONDAHL, *Director of Research and Engineering, Union Switch & Signal Company*
GEORGE R. HARRISON, *Director of Applied Physics, Massachusetts Institute of Technology*
MAURICE HOLLAND, *Director, Division of Industrial and Engineering Research, National Research Council*
A. W. HULL, *Assistant Director, Research Laboratory, General Electric Company*
LOYD A. JONES, *Physics Department, Research Laboratory, Eastman Kodak Company*
M. J. KELLY, *Director of Research, Bell Telephone Laboratories*
A. W. KENNEY, *E. I. du Pont de Nemours & Company*
PAUL E. KLOPSTEG, *President, Central Scientific Company*
C. H. KUNSMAN, *Chief, Fertilizer Investigations, Bureau of Chemistry and Soils, U. S. Dept. of Agriculture*
G. M. J. MACKAY, *Director of Research, American Cyanamid Company*
JOHN P. MAGOS, *Testing Engineer, Research Testing Laboratory, Crane Company*
ARCHIBALD F. MESTON, *Research Corporation*
GEORGE B. PEGRAM, *Dean of the Graduate School, Columbia University*
H. M. RANDALL, *Professor of Physics, University of Michigan*
F. K. RICHTMYER, *Dean of Graduate School, Cornell University*
HERMAN SCHNEIDER, *Dean, College of Engineering and Commerce, University of Cincinnati*
JOHN T. TATE, *Dean of the College of Science, Literature and the Arts, University of Minnesota*
JOHN WULFF, *Assistant Professor of Physics, Massachusetts Institute of Technology*
HENRY A. BARTON, *Director, American Institute of Physics*

Substitutes

GEORGE V. MCCAULEY, *Corning Glass Works (for J. T. Littleton)*
DAYTON L. ULREY, *Consulting Physicist, Westinghouse Electric & Manufacturing Company (for L. W. Chubb)*

Guests

HOMER L. DODGE, *University of Oklahoma*
R. CLIFTON GIBBS, *Cornell University*
ELMER HUTCHISSON, *American Institute of Physics*
L. W. MCKEEHAN, *Yale University*
A. R. OLPIN, *Kendall Mills, Paw Creek, N. C.*
F. A. SAUNDERS, *Harvard University*
L. B. TUCKERMAN, *National Bureau of Standards*
A. G. WORTHING, *University of Pittsburgh*
D. L. WEBSTER, *Stanford University*

II

Secretary's report

The Secretary reported that the last meeting of the Council had been made the subject of a report published in the *Review of Scientific Instruments* for March, 1936, of which reprints had been sent to the members. This report was allowed to stand in lieu of other minutes. The Secretary read the following resolutions voted by the Governing Board of the Institute at its meeting March 21, 1936:

1. That the Chairman of the Institute be authorized without further action of the Board to appoint the Council anew each year after it, along with other Institute committees, retires.

2. That the Council be empowered to appoint subcommittees, including an executive committee with power to act for it, and also to hold meetings, call conferences, raise moderate funds for its own expenses, and engage in

other activities within the scope of the preamble* and consistent with the broad policies of the Institute and the Founder Societies.

3. That the Council be instructed to arrange through its executive committee or otherwise to nominate, at the request of the Chairman of the Governing Board, persons for appointment or reappointment to the Council, with the provision that each Associate of the Institute is to be invited to nominate a member.

Commenting on these, the Secretary pointed out that the Executive Committee should now prepare a ticket of nominations for membership in the Council for the year 1937 and that this ticket should be presented to Dr. John T. Tate, Chairman of the Institute, for action at the time of the Board meeting early next spring. The Secretary explained further that the joint meeting of the Founder Societies of the Institute scheduled to take place the next three days had been organized under the supervision of the Council. The Council had been assigned the job of planning and arranging that meeting. Its Executive Committee actually took that responsibility for the Council and delegated it to a local committee in New York of which Dr. O. E. Buckley was Chairman. The Secretary mentioned one further item, namely, the Symposium on Industrial Physics scheduled for the following afternoon. (The papers in these several sessions will be published. Plans for their publication in periodicals of the Institute, including the new *Journal of Applied Physics*, are completed and when such publication has taken place the papers will be assembled in a book entitled *Physics in Industry* which will be issued as soon as possible. The book will be on sale through the Institute.)

On motion by Dr. Eppley the report of the Secretary was approved.

III

Chairman's report

The Chairman then reported, stating that inasmuch as various subjects were to be taken up later in the agenda he would refer in general terms only to several of these. Speaking for the Executive Committee, he mentioned that one of the principal duties assigned by the Council was to study the education of physicists for industrial positions. There had been a very great amount of resulting activity in the shape of discussions at various meetings throughout the country and in correspondence with a large number of physicists. Certainly a very large amount of interest had been aroused. He pointed out that the meeting the next two days was largely devoted to this subject.

In order further to assist those responsible for setting up university curricula, he continued, two committees had been formed to prepare reports on the basis of this great amount of discussion. These reports would aim to translate the main ideas advanced into a coordinated plan for effective action. The first of these committees is a committee of the American Association of Physics Teachers with the following personnel:

P. I. Wold, *Chairman*	G. R. Harrison
H. A. Barton	A. W. Hull
G. A. Campbell	F. G. Keyes
Harvey N. Davis	R. von Nardroff
H. L. Dodge	W. Wilson
Lee DuBridge	A. G. Worthing

The second committee is a committee of this Council appointed at the suggestion of the Secretary and of Dr. D. L. Webster, President of the Teachers Association. The Chairman announced the personnel of the committee as follows:

Paul E. Klopsteg, *Chairman*	George V. McCauley
Saul Dushman	H. B. Williams
E. A. Eckhardt	Earl D. Wilson
Ross Gunn	T. D. Yensen

The Chairman also said that another side of the Executive Committee's responsibility was to prepare the field in industry for men who have been trained. To do this it would be necessary to gain the interest of individual industries in physical research. This purpose would be furthered by the program planned for the next two days. (Here the Chairman referred to the symposia discussing the application of physics to a number of major industries.)

IV

The Chairman referred to the action of the Council at its last meeting recommending that the American Physical Society form a division

* The preamble, not read by the Secretary, referred to the functions of the Council as accepted by it in its meeting November 1[illegible], 1935 (see report of that meeting) and, further, expressed the desire of the Governing Board to give the Council permanent standing and special powers as a committee of the Institute.

of applied physics. He reported that this action, when further discussed and conveyed to the American Physical Society, had resulted in a somewhat different point of view calling for vertical divisions by subject matter rather than any horizontal division between pure and applied physics in the Society. The Physical Society had appointed a committee with Dr. M. J. Kelly as Chairman to study this question and advise the Society. A report to this Council by Dr. Kelly would be made later. Growing out of this committee's considerations, plans had been completed for the publication of a new *Journal of Applied Physics* in place of the old *Physics*. This new journal would be published cooperatively by the Institute and the Physical Society under the editorship of Dr. E. Hutchisson who was here to tell the Council about the details of its scope and policy. Preparations for this journal had been in the hands of two cooperative committees—one for the Institute and one for the Physical Society.

The Chairman urged that the projects to be reported upon receive the careful consideration of the members of the Council through discussion or later correspondence and asked also for suggestions concerning desirable additions to the personnel of the Council for next year. He pointed out that since the Council is an advisory rather than an executive body, it would be feasible to have a rather rapid turnover in membership, possibly retiring one-third each year.

V

Committee on Applied Physics

At the request of the Chairman, Dr. Kelly presented a report of which the following is a digest:

The Council last year considered the training of men, publication facilities and types of organization for applied physics. Covering the last two items it was resolved to recommend the formation in the American Physical Society of a Division of Applied Physics which would govern itself within limits, arrange programs and editorially direct the journal *Physics*. This action of the Council came informally to the attention of the Council of the American Physical Society on November 29, 1935. The latter Council welcomed consideration of the matter and set up a Committee to report regarding the desirability of forming such a division, its appropriate functions and details of organization. The Committee formed has the following members:

M. J. KELLY, *Chairman*	G. R. Harrison
H. A. Barton	L. A. Jones
O. S. Duffendack	J. T. Littleton
Saul Dushman	W. L. Severinghaus
L. O. Grondahl	J. T. Tate

Early in 1936 the Committee reported as a result of considerable study that (1) there were very many members and nonmembers of the Physical Society who would welcome the establishment of a suitable place for applied physicists, (2) there was no unanimity as to what this place should be, (3) there is little support for the formation of new societies and, therefore, a development within the Society would be welcome and (4) there was a body of opinion that no division should be formed but rather that applied physics material should be developed and presented with the allied pure physics material.

The Committee therefore recommended that for the present a steering committee be set up to arrange applied physics papers at meetings and to continue studying the needs of the applied physicist. The Council directed the Committee to assume this task. One of its steps since was to arrange the joint session with the Institute of Radio Engineers in Washington last April. The Committee also cooperated in the arrangement of the Symposium on Applied Physics to be held tomorrow and the next day. Further plans are in preparation.

The Committee found a much more fully developed viewpoint in the matter of publications, to wit: that present media were inadequate and that a suitable journal for the applied physicist was greatly needed. This was reported to the Council of the Physical Society with general recommendations as to the type of journal needed. A committee of three—M. J. Kelly, F. K. Richtmyer and J. T. Tate—was requested to work out a detailed plan for such a publication. With the help of offices of the Society and the Institute such a plan was prepared and received the approval of the Council. (Dr. Kelly left it to Dr. E. Hutchisson, who will edit the journal, to report further regarding its editorial scope and features.)

It is believed that a journal will result from these preparations which will really interest applied physicists, serve as a nucleus to attract their interest to the Physical Society and aid in providing experience on the basis of which to appraise future organization needs. It is desirable that this growth of interest be spontaneous and unforced. It should certainly come before any fixation of organization because support of the latter is entirely dependent upon the degree of interest.

VI

Dr. Fletcher raised the question of membership fees referring to the possibility that a new type of membership in the Physical Society might further the general aim of stimulating interest in the Society on the part of industrial physicists.

Dr. Kelly replied that this had been and still was the subject of very careful thought but that there were very considerable difficulties in the

establishment of differential dues. He mentioned, however, that members would have a free choice as to which of the publications of the Society they received under their memberships. There was further discussion by several members of the matter of membership, local sections of the Society and the flexibility of the Society's constitution.

VII

New Journal of Applied Physics

At the request of the Chairman, Dr. Hutchisson presented essentially the following statement:

Preparations for the first issues of the *Journal of Applied Physics* were begun in August. The American Physical Society had agreed to turn over to the American Institute of Physics the good will and subscription list of *Physics* as a basis upon which the new journal would be built. Many rather general considerations as to format, number of pages, paper stock, cost and other questions of a similar nature were discussed. To eliminate competition with other Institute journals, it seemed inadvisable, at least for the present, to carry independent advertising in the new journal. The simplest solution of the advertising question seemed to be to include the same advertising pages as are now carried in *The Review of Scientific Instruments*. Doing this would have the effect of increasing the numerical coverage for a given advertiser and also of increasing the range of reader interest. Because of this fact it is hoped that a sufficient increase in advertising income will occur so that the division of the advertising income between the two journals in proportion to their circulation will not affect adversely *The Review of Scientific Instruments*. This decision with respect to the advertising required that the format of the new journal be the same as that of the other journals of the Institute. There are many other advantages in such a format, especially since the present format allows very economical printing in forms of 16 or 32 sheets. One disadvantage of the standard format is the restriction on the size of the cuts and the general layout of the type material and figures.

As to the kind of articles to be published, there are several considerations which may be used as criteria. They must be popular in the sense that one does not need to be a specialist in the particular field being discussed in order to understand the article. For instance, an article on vacuum tubes should be written so as to be understandable to a biophysicist. On the other hand, articles on biophysics should be readable to one who is specializing in vacuum tube research. Another criterion which has been suggested is the degree to which the given article tends to stimulate further applications of physics. Since one of the primary aims of a journal of this type is to bring about closer co-operation between those doing pure research in university and other laboratories and those working in industry, this latter criterion seems a particularly valuable one. In every way possible, it will be the purpose of this journal to enhance physics as a profession by bringing to the attention of industrialists the possibilities of pure and applied research in physics.

The following classes of articles are planned for the initial issues of the journal:

1. Papers dealing with the application of physics in particular industries or in borderline sciences.
2. Articles describing in some detail the personnel and organization of research laboratories. These articles will emphasize individuals in the laboratory and their work rather than buildings and equipment.
3. Articles of a biographical nature, profusely illustrated and, in general, concerned with those in applied physics.
4. Articles dealing with special physical tools such as the spectroscope, x-ray tube, vibration meter and so forth.
5. Summaries of prominent research papers in other journals together with biographical material about the authors.
6. Contributed original papers. These papers would be of the research type and be approximately the same style as those previously published in *Physics*. An effort would be made to change the style of these papers so that they would conform a little more closely to the general character of the journal.
7. Advertisements. Advertisements seem to have "reader" interest and are probably essential to the proper character of the journal. The "Current Literature of Physics" will also be included with the advertising pages in the same manner as is now done in *The Review of Scientific Instruments*.

The contents and specimen pages of the new journal were indicated in a prospectus which was passed to all those present. It was then explained that the character of the *Journal of Applied Physics* is just being formed and that the plans for its content are very flexible. The content will be changed from time to time as the desirability of changes is indicated by the readers' interests. Since this journal arose principally through the efforts of the Advisory Council, suggestions from its members, in particular, were requested.

Discussion following Dr. Hutchisson's report served to clarify further the plan for the new journal. The list of associate editors was asked for and read. They are the following:

R. M. Bozorth	C. H. Kunsman
D. W. Bronk	J. T. Littleton
W. P. Davey	L. L. Nettleton
O. S. Duffendack	W. B. Nottingham
Saul Dushman	A. R. Olpin
A. Stuart Hunter	J. T. Tate

The question was asked whether the journal would continue the service of *Physics* as official publication medium for the Society of Rheology. No definite answer was possible at that time. (Note by Secretary—The Executive Committee of the Society of Rheology later voted to continue with the new journal the arrangement under

which regular members now receive *Physics*. The new journal's character, however, is unsuited to a formal continuance of the cooperative editorial arrangement between the two Societies.)

Dr. Kelly added that every care would be taken to avoid conflict of scope with the other journals published through the Institute. While this had particular reference to the journals of the Optical Society and the Acoustical Society which already serve important fields of applied physics, much consideration had also been given to cooperation with *The Review of Scientific Instruments*.

Dr. Hutchisson mentioned conversations he was having with Dr. Roller, Editor of the American Physics Teacher, to prevent undesirable overlapping with that journal.

VIII

Popular book on applied physics

On request from the Chairman, Dr. Harrison presented a report of which he later prepared the following outline:

As a part of the present effort to make the public more physics conscious, it is purposed to write a popular book which will attempt to do for physics the sort of thing that Slosson's *Creative Chemistry* did for that science. The objectives may be stated as:

(1) To help make the public more aware of the accomplishments of physics by an enjoyable process;
(2) To gather together the various fragments and offshoots of physics which should still be cataloged as physics, and present them as a unit, emphasizing the interdependence of fundamental and applied physics;
(3) To show how the various physical and natural sciences are interrelated, and how great is the importance of mutual collaboration;
(4) To emphasize how greatly the social structure and the very course of civilization is influenced by the development of fundamental and applied science.

In a popular book these objectives must be submerged so deeply that there is no evidence of these purposes on the surface.

It is hoped to accomplish this by writing a popular book of about 100,000 words, containing a thoroughly up-to-date discussion in simple language of the developments of applied physics. The style will be anecdotal rather than one requiring logical continuity of thought. An attempt will be made to hold the reader's interest with the subject matter and with an imaginative treatment without straining for effect, forced humor, or superficial style.

An attempt will be made to show the interrelation of discovery, invention, and research, without using the cut-and-dried historical method.

It is suggested that chapters be included on the accomplishments of physics in the communication field, transportation, oil production, power, crime detection, refrigeration and air conditioning, building construction, and various other industrial fields, with two or three chapters on contributions of physics to related sciences, which have in turn applied them to developments of public interest.

An especial attempt is to be made to have an attractive title and interesting chapter headings. It is felt that the title should be brief and should probably not contain the word "physics," except possibly in a subtitle.

A number of publishers have expressed an interest in such a book, but it is felt that the question of publishers should not be considered seriously until the book is well along. The possibility of direct publication by the American Institute of Physics should be carefully considered.

If this book should be successful, it will be of special importance to have accurate up-to-the-minute information about interesting developments in various fields in which physics is applied. The members of the Advisory Council, and physicists in general, can contribute greatly to what is believed to be a worthy cause by sending in suggestions regarding the book to the American Institute of Physics, suggesting material which should be written up, possible titles, chapter headings, or anything relevant to the production of such a book.

It is hoped that the book will give a demonstration of the contribution of physical research to society, which all physicists will agree is an important one, and it is hoped that at the same time the book will be readable and of interest to the general public.

At Dr. Harrison's request, the Secretary referred to the background of the idea for the contemplated book recalling that the Council

had decided such a book should be written by vote at its last meeting. The need was visualized as a situation in physics somewhat analogous to that in chemistry fifteen or twenty years ago. At that time, E. Slosson's *Creative Chemistry* appeared and focused attention on the practical industrial value of the science. Its success was due largely to its style and readability as well as the point of view of the author. After considering a number of possible authors, the Executive Committee of the Institute had come to the conclusion that Dr. Harrison not only possessed the right point of view but was capable of commanding the necessary style for such a book to interest the desired audience.

Dr. Harrison stated he believed a book could be written which would be both dignified and yet popular in expression. He commented on various possibilities as to length, mode of publication, etc., and introduced the question of a suitable title. There was a very interesting and rather general discussion on the use of the word "physics" in the title. Members recalled some of the objections and advantages of the word "physics" for the name of the science and of the name "physicist" for its professional following. The discussion brought out two alternatives. (a) To recommend changing the name of the science, and (b) to continue endeavors to give the name "physics" the connotation wished for it in the minds of the public. Mr. Buffum remarked that at one time the name "chemist" was considered in the same fashion as the name "physicist" was here being considered and pointed out that members of the chemistry profession have little fault to find with it at the present time.

Further points brought out in the discussion were that suggestions should also be received if possible regarding the book from the ranks of possible readers. This might be done through preliminary publication of some part of it in a lay journal of broad circulation.

Several resolutions were offered regarding the use of the word "physics" in the title or sub-title but no action was taken. It was the general sense of those present that the author should be left quite free in this respect and that the Council should not make any general recommendation calling for a change in the name of the profession.

IX

Training physicists for industrial positions

Upon invitation of the Chairman, two prepared reports on the university view of the task of training physicists who later would fill industrial positions were presented by Dr. Randall and Dr. Worthing. Following are drafts of these reports prepared by the authors:

Dr. Randall:

In his note inviting me to speak at this meeting the Secretary stated that, the comments of the industrialists concerning applied physics having been presented at earlier meetings, it might be well to hear the reactions of the universities to them. Before beginning such a discussion I should like to express my appreciation of the wholehearted response of industrial physicists to this program on applied physics which the Institute of Physics is developing. I am sure that I speak for all college men when I say that our colleges and universities will welcome their suggestions leading to the better preparation of graduates in physics for positions in industry.

To come to the immediate question—what are the outstanding deficiencies found in the men trained in physics when they begin industrial work? Those most frequently stressed are these: a lack in personality, an inability to adjust themselves readily to industrial conditions, and an over-emphasis on modern physics in their training at the expense of classical physics. The lack of adaptability may be shown by a wrong point of view, or by a lack of technical skills common to industrial practices, or by both. It was often expressed in terms of the greater adaptability of the engineers and the chemists.

To a certain extent the universities have recognized these deficiencies in their graduates and I will limit my discussion to a statement of what is being done to overcome them and to point out directions in which progress might be expected. For engineering students with bachelor degrees, the four years in the applied physics of engineering should fit them readily to adjust themselves, that being, in fact, a main objective in their training. While it is not so obvious why chemists so often prove superior to physicists it becomes understandable when it is considered that throughout his training in chemistry the future chemist is being instructed constantly in physics, and in the use of physical equipment. The stronger the training in chemistry is, the more likely is it to have been based upon the underlying principles of physics. Such training is very effective in applied physics, in the field of chemistry. So the chemists as well as the engineers bring to their professional careers four years of intensive work in the applications of physics to their fields of specialization. The physicist has had no such training in the applications of his science and it does not follow that his lack of aptitude shows inability to function in industry, but rather that his training has been deficient in the technique of application. This fact has been recognized by the schools for some time and a number of institutions have established courses in

engineering physics, intended to rectify this lack. At Michigan there is a bachelor's degree in engineering in physics. Students in this course have, with those in other engineering courses, a quite parallel schedule for the first two years. In this time such subjects as the following are taken—English, 8 hours; mathematics, 18 hours; drawing, 8 hours; physics, 10 hours; chemistry, 8 hours; metal processing and chemical engineering, 5 hours; economics, 6 hours. The last two years have among others, the subjects, mechanics, 13 hours; heat engines, 4 hours; electrical engineering, 8 hours; physics, 15 hours; options in physics and mathematics, 12 hours; general electives, 12 hours. This engineering degree in physics has attracted a high grade of students who have been generally successful in their subsequent careers. By taking an additional summer's work, engineers in physics can obtain an engineering degree in electrical engineering.

For many industries the training in physics possible in a four year undergraduate course is quite inadequate, since they wish to apply, when necessary, everything physics can offer, even the most advanced, certainly in classical physics and often in modern physics. This means men with doctor's degrees or the equivalent. Assuming that their formal training has been adequate in the sense that their records as students are of a high order and that in addition they have the originality and the mastery of the subjects treated to permit them to solve such problems, experimental or theoretical, as develop in university research, what still is necessary for a successful industrial career? Apparently it is essential that they bring to this work the right point of view, a flair for the practical application of their subject, and a certain number of industrial techniques. If the student seeking a higher degree in physics comes with a bachelor's degree in engineering he is quite certain to meet these conditions and can devote his time to his graduate work which would be mainly in mathematics, physics, and chemistry. If, however, he comes from a literary college then his graduate work in the above subjects can be supplemented, as has been suggested, by such courses in engineering as his field of specialization indicates. This procedure is followed at Michigan and I do not doubt at other universities. There is, however, a limitation to the number of undergraduate courses in engineering a man can take in physics if he is to be highly prepared in advanced physics.

For eight or nine years the department of physics at Michigan has carried on industrial research in collaboration with the department of engineering research. About half of the staff have at one time or another been engaged in such work, two of them quite continuously. Each research project which is accepted is carried on by a research physicist having no teaching duties, who gives his entire time to the work. The function of the staff man is primarily that of consultant. It has been possible in this manner to carry on continuously several industrial researches without impairing the regular work of the department which is in pure physics as distinguished from applied physics. Frequently students going into industry act as research assistants in these industrial projects, obtaining direct experience in this type of research, as well as the industrial point of view. Pure and applied physics in this way have been intimately associated at Michigan for a good many years to the apparent advantage of both. Such opportunities are not generally available, however. Working in industrial plants summers or alternate semesters has for its purpose the acquirement of this desired experience. Such opportunities should be increased and one of the purposes of this council might well be to consider the conditions under which such part-time work in industry during college training might be greatly amplified.

As to the actual instruction received in college, the principal criticism has been that a thoroughgoing foundation of classical physics has too frequently been sacrificed for a superstructure of modern physics. There is justice in this complaint, particularly as classical physics generally finds a larger field of application in the industry of today than does modern physics. Undoubtedly as a result of these considered opinions of industrial leaders, fundamentals will receive their proper recognition in the training of our advanced students in the future. For those with bachelor's degrees only, these strictures about modern physics do not apply naturally and, except that more chemistry would be desirable, it appears that the training now received is generally satisfactory and that failures are to be charged more often to a lack of the right type of personality than to a deficiency in training.

In industries with well developed research laboratories where the various lines of investigation are under the direction of competent men there is room for physicists whose training may vary over wide limits. Every college man, however, knows of many instances where students of meager training or of mediocre ability have by personal application obtained responsible positions in industries beginning to introduce the methods of physics in their organizations. While such occurrences may speak well of the personality and enterprise of the student the almost certain dissatisfaction likely to result in such instances will be a serious deterrent to the rapid expansion of physics in industry. If the industrial physicist is to be a member of a reorganized profession then there must be professional standards which have to be met and which are so generally recognized by industry that no inadequately trained man can hope for employment. To aid in establishing such standards and to acquaint industry of their necessity, are, I believe, proper activities of this council.

Dr. Worthing:

In the starting of a program involving the education of physicists for industry as with the starting of any enduring program, the establishing of a policy is one of the first and most important undertakings. Much depends upon its formulation and its maintenance.

In drawing up a policy for the education of the industrial physicist, three questions arise. They are:

(1) What general classes of industrial work are there which require the training characteristic of the physicist more than or as much as that which is characteristic of the chemist and the engineer? In other words, toward what kinds of jobs in industry ought the physicist to strive?

(2) What training will best fit the physicist for such positions and for his place in his community? This second question involves his course of study and many other subsidiary questions.

(3) How can physics students be given an opportunity to become interested in industry and to maintain that interest in the midst of instructors who are generally non-industrially minded? In other words, how may an academic physics department create and maintain a proper morale for the industrially minded as well as the non-industrially minded student?

At the University of Pittsburgh we have had to answer these questions on different occasions, when we started the course in "physics and engineering" in the School of Engineering and at times when we considered the futures of nonengineering students majoring in physics.

Of those who go out from the University of Pittsburgh with bachelor's degrees in physics, those who have taken the course in "physics and engineering" are undoubtedly best prepared for industrial positions. It is, therefore, appropriate to give the answers to the main questions involving policy, which arose at the time that this course was established.

To the first question concerning the general classes of industrial work for which the training characteristic of the physicist is especially suitable, the logical answer seems to be:

(1) Research work of the nonroutine, nontesting, fundamental type, where information is sought more from the point of view of ultimate value than of immediate use and as is carried on by the Westinghouse Research Laboratory, the Mellon Institute, the Gulf Research Laboratory, the General Electric Research Laboratory, the Bell Telephone Laboratories, the National Bureau of Standards, the Bureau of Mines, the Federal Bureau of Crime Detection and the Smithsonian Institution.

(2) Consultation with regard to facts and principles which are likely to be outside of the field of the chemist and the engineer such as is required in the Patent Department and in supervisory work in many industrial plants.

In answer to the second question, namely, what training will best fit the physicist for such positions in industry and for his place in his community, the following curriculum was drawn up:

30 credits in physics
30 credits in engineering with emphasis on electrical engineering, and with individual courses in graphics, machine design, mechanics of materials, power plants and metallurgy
8 credits in chemistry
16 credits in mathematics, through calculus
6 credits in English
9 credits in German or French
4 credits in other required courses
34 credits in electives

137 credits, required for graduation

Just as in the case of students majoring in engineering or in chemistry, special training is needed for many research problems. Usually students majoring in "physics and engineering" take courses in physics beyond those specified. Most of them take differential equations. Many take additional courses in chemistry, usually physical chemistry. With such preparation, we think them well prepared to accept many industrial positions of the desired type.

Attention should be called to a fact of which we are quite proud, namely, that about one-fourth of the courses taken by our "physics and engineering" students are elective. In this respect our schedule is probably unique among those to be found in engineering institutions in the United States. This feature made it possible, for example, for one whose special interest was stage lighting, and another whose special interest was music, to take courses in line with those interests. The elective courses also give the students opportunity to broaden their outlook by taking courses which are unrelated to their technical work and thus to adapt themselves better to their community life.

Of those who go out into industry after having taken graduate work, those who have majored in physics and engineering do not cause much concern. They are familiar with necessary operations, they are able to make reasonable and readable designs of apparatus and to operate, repair and modify electrical machinery and to talk intelligently on engineering subjects. On the other hand, some of the physics graduates of the ordinary letters and science colleges are unfamiliar with the operation of a lathe and are unable to read rapidly mechanical drawings or make simple sketches of machine designs. Courses in engineering and chemistry which are normally undergraduate are often recommended to these students.

As to the physics and mathematics courses for the graduate industrial physicist, no general statements may be made since so much depends upon the individual. In all cases we stress the so-called classical courses in mechanics, thermodynamics, electricity and magnetism, and light. We offer semi-industrial courses in electricity and magnetism, vacuum tube theory and practice, high temperature measurements and acoustics. Further, we offer courses in x-ray analysis and spectroscopy with experimental work in departmental laboratories where industrial investigations are carried on.

To the third question, "How can physics students be given an opportunity to become interested in industry and to maintain that interest in the midst of instructors who are generally nonindustrially minded," we, I fear, did not supply a complete answer. However, we have taken the initiative, under Dr. Hutchisson's leadership, in holding an open conference on industrial physics, and will continue with this policy. Dr. Hamor, Assistant Director of Mellon Institute, has suggested that we "give attention to a series of lectures by industrial physicists in the Pittsburgh district." Perhaps that is what we need to do. We are still new at the game of industrial physics for our graduate students, and our procedures are subject to change.

Another contribution which I may offer to this discussion is somewhat indirect. Following our first industrial conference a year ago, we sent requests to several directors of industrial research for suggestions as to how our graduate curriculum might be modified in the interest of industrial physics. (Dr. Worthing read at this point a number of excerpts from letters received in response to these requests.)

The letters received were full of valuable suggestions and we are making use of most of them. But after all I can't but believe that there is much in the thought so well expressed by Dr. Weidlein, "I have a very strong feeling that it does not make any difference whether a man is prepared for teaching or for industrial research." To this, I think he might well have added the belief that industry is much more interested in innate ability than in any particular preparation which the student receives provided it is not too far removed from the kind of work into which he is going.

The prepared discussions of Dr. Randall and Dr. Worthing were followed by general discussion in which most of the members of the Council took part and in the course of which a great many considerations were advanced. These considerations included such points as the following:

(1) Components properly to be included in the education of a physicist viewed on the basis of the requirements of industry;

(2) The necessity for an industrial physicist to have a high morale, i.e., an enthusiasm for industry rather than any doubt or fear of his place in industry;

(3) The necessity for educators to avoid blindly accepting industry's idea of its requirements which may be, and often is, short-sighted;

(4) The fact that in many cases earnings have come to industries as the result of contributions from science which they did not seek;

(5) The necessity for teachers to endeavor to prophesy what kind of men industry will want twenty years from now;

(6) The difficulty of telling industrial organizations what they want, particularly in connection with their own jobs;

(7) The need of many rank and file physicists as well as some leaders;

(8) The possibility that the engineer is the proper delegate of physics to industry in this grade;

(9) The view that the best men in industrial work are engineers at heart who know some physics;

(10) That education does not do much more than indicate what kind of a man one is dealing with;

(11) That physicists must be human and able to sell themselves in industry, that in this they differ from physicists in scholastic work where they do not need to sell themselves;

(12) The fact that the great majority of industrial laboratories have small staffs including only one or two physicists;

(13) That in small laboratories the physicists need a broad training rather than a specialized training only adaptable to the work of large organizations;

(14) That it is extremely important to emphasize facility in self-expression.

It was also suggested that a test curriculum be set up to serve as a basis for concrete discussion at future meetings. The Chairman pointed out that the two committees announced earlier in the meeting were expected to submit reports which should furnish a basis for action by universities and for criticism from physicists in general.

X

At the request of the Chairman, Dean Schneider of the University of Cincinnati described the procedure recently inaugurated in that university for cooperative training of physicists. Under this procedure pairs of students spend part of their time in industrial organizations and part of their time in the university. This procedure meets with the satisfaction of research executives of the companies cooperating and was set up largely as a result of preliminary conferences with these executives regarding the best method of training physicists for industrial positions.

XI

New business

After completion of the items on the prepared agenda, the Chairman called for new business. Dr. Davey presented a proposal for an employment information service which he urged the Council to recommend for adoption by the American Institute of Physics. The idea for this service was to supply industries with a full coverage of those who desired employment at any time so that an intelligent selection of qualifications might be made. Details worked out included standard information and recommendation blanks, registration fees, placement fees, publication of lists, financing, etc. It was proposed that the service be limited to members of the Founder Societies of the Institute.

On motion of Dr. Fletcher, it was voted to refer the matter of an employment information service to the Governing Board of the Institute of Physics without further discussion at the moment as to whether or not in the Council's opinion such an information service could accomplish useful results.

The Chairman then expressed the appreciation felt by all concerned with the Institute and the Council of the work of those who had reported and of the attendance of the members at this meeting.

HENRY A. BARTON, *Secretary*

The Physicist Gets Air Minded*

By CLARK B. MILLIKAN

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Pasadena, California

THE field which I will discuss in this paper has several features which distinguish it from the other types of industrial activity forming the subject matter of the symposium on Physics in Industry. In the first place, air transportation is much the youngest of any of the fields under discussion, while in its commercial aspects it is truly an "infant industry." It may be in part a consequence of this fact that the aeronautical industry has been, and to a large extent still is, a "young man's game." Not only the engineers, but also the executives are, on the average, very considerably younger than are the men occupying analogous positions in most industrial fields. As a result the industry has grown up with an unusual absence of tradition and experience. This has meant that development had to be based to a remarkably large extent on the results of research and theoretical calculation. A second important feature of aeronautics lies in the extreme narrowness of the margin between success and failure and in the terribly serious consequences which may result from failure. This has placed a tremendous premium on exactitude and has greatly stimulated activity in research of all kinds. Since practically all of the problems of aeronautics are essentially physical in nature (except for certain psychological and aesthetic questions which belong properly in the realm of sales departments), it is apparent that aeronautics has furnished an unusual field for the intensive use of applied physics. Since time is limited, I shall merely mention a few of the various branches of physics which have been applied to aeronautical problems, and shall then discuss in more detail the use of that branch which seems to me most fundamental for the general problem of flight.

Thermodynamics is, of course, essential to any development of fuel burning power plants, both in studying questions of combustion, etc., and also in the difficult problems of heat conduction and transfer. Both questions are especially serious in aeronautics because of the constant pressure for large power output with low weight and fuel consumption in connection with aircraft engines. Thermodynamics is also an intrinsic element in "Physics of the Air" upon which is based all of the modern synoptic meteorology. Without this aid air transport, as we now have it, would be almost an impossibility since weather troubles would make air services so unreliable as to eliminate most of their economic usefulness. Electricity and magnetism have been largely used in connection with aircraft instruments and radio, two of the elements which are vitally important to satisfactory air travel. Acoustics has recently been actively applied to the problems of noise reduction. These and many other instances of the applications of physics to detailed aeronautical problems could be cited. However, the one branch of physics which is of truly basic importance to the field of aeronautics is that of mechanics, and it is to this branch that I propose to confine my attention today.

The history of the relation between scientific mechanics and engineering in general is an extremely interesting one, and in its more recent phases has been much influenced by aeronautics. In classical times, and also during the much later period when the foundations of mechanics as we know it were being laid, the designing engineer and the scientist seeking to formulate the laws of nature were often one and the same person. Indeed many of the basic laws of me-

* Presented at the meeting of the Founder Societies of the American Institute of Physics, October 29-31, 1936 in New York.

chanics were discovered as a result of problems which arose in the attempt to answer specific engineering questions. Unfortunately, however, this close contact between science and engineering was not maintained. The classical laws of mechanics as they were finally formulated in the middle of the eighteenth century had an elegance and generality which attracted to them mathematicians and other theoretical workers. For nearly one hundred and fifty years their efforts were largely devoted to obtaining beautiful, formal solutions of the general equations for all kinds of special cases. In order that such solutions could be obtained, it was, however, necessary to make important simplifying assumptions.

Perhaps the best example of this procedure is furnished by hydrodynamics where the neglecting of viscosity seemed essential if exact solutions of the hydrodynamical equations were to be obtained. For nearly a century and a half, therefore, almost all scientists considering the problems of fluids worked with frictionless fluids and developed the beautifully exact and complete field of classical or "perfect fluid" hydrodynamics, one of whose characteristic features was that a body moving with no acceleration through a uniform fluid experienced no resistance. During this long period the engineer had to solve actual problems of practical hydraulics, and these problems in general had as their crux the overcoming of resistances in fluid motion which were found by experiment to occur. For such problems the scientist, whose theories did not even indicate the existence of these resistances, was clearly of little help. It is not at all surprising that the engineer soon gave up even the hope that analytical science could be of any assistance to him. Engineering hydraulics as a consequence developed as an essentially empirical subject, filled with countless numerical factors, and entirely divorced from the mathematician's hydrodynamics, and even from any basic and rational scheme or framework. Although this condition was perhaps worst in connection with fluid problems, it was very similar in the fields of elasticity, structures, etc., indeed in practically all of the fields which should be included under the banner of mechanics.

There is a very interesting anecdote which is told concerning the beginnings of a new rapprochement between the practical and the theoretical groups. The story is well enough authenticated to merit a mention of it here, especially since it has a particular significance for Americans. At the time of the Chicago World's Fair near the end of the last century, the famous German mathematician, Felix Klein, visited the United States and in particular the World's Fair. He was enormously impressed by the wealth of raw materials, the energy, and the mechanical aptitude which he found in this country. In fact he was so impressed as to be much afraid that within a few years Europe could no longer compete with this rich new country. The conclusion to which he finally came was that in order for Europe to maintain her position she must develop her engineering activities to such a high level of efficiency as to compensate for the natural advantages of America. It was his conviction that this could best be done by applying the most advanced scientific technique and ability of his country to the problems of engineering. Upon his return to Goettingen, he accordingly began to build up a school of applied mathematics and mechanics, and gathered around himself an extraordinary group of brilliant young scientists whom he trained to carry on the work. Not only did he develop a new interest and point of view for mechanics but also exerted such an influence on his young colleagues and students that today they are scattered all over the world as leaders in the great effort to bring engineering and science ever closer together. I need mention only a few names from this group to illustrate their widespread influence today. Prandtl, von Kármán and Timoshenko are perhaps the best known in this country, but they by no means exhaust the list.

In view of the tremendous influence which Klein and his school have exerted on modern engineering, it is perhaps worth attempting to put into a few sentences the essential elements in their method of treatment of the problems of mechanics. In the first place the emphasis is shifted from efforts to obtain general, formal, and rigorous solutions of physical equations simplified so that such solutions are possible, to attempts at getting approximate solutions of more specialized equations in which, however, all of the

essential physical elements of a particular problem are included. In the second place the interest of the scientist working in these fields is altered. He can no longer be trained solely in pure mathematics and abstract theoretical physics, but must have enough feeling and experience with the engineer's problems to be able to attack them with some insight into the relative practical importance of the various elements concerned.

At the time that the famous Goettingen school under Felix Klein was just reaching its zenith, the art of heavier-than-air flight was emerging from the realm of speculation into that of actuality. In spite of some scientific attempts, it must be admitted that most of the early advances in actual flying were made by inventors and craftsmen rather than by theoretical or even experimental scientists. It soon became very apparent, however, that a much deeper knowledge of the underlying physics of flight would be required before any great forward steps could be taken. This was exactly the type of scientific problem which Klein's group was fitted to tackle, and they lost no time in getting to work. A large percentage of the group during the early years of the present century investigated problems connected with aeronautics, and their students soon occupied many of the leading engineering positions in the airplane industry. In this connection, the lack of any large body of empirical knowledge and the absence of any considerable group of old and conservative engineers resulted in an amazingly rapid acceptance of the theoretical or scientific mode of approach to practical engineering problems. Because of their greater inertia, other branches of engineering were much slower to adopt the new points of view. Only fairly recently has hydraulics begun to use at all widely the hydrodynamical principles developed originally for aeronautical applications. Similarly, civil engineering in the last few years has begun to take advantage of the radical structural ideas which were conceived in connection with aeronautical problems.

In order to illustrate the growth of the scientific spirit in engineering and to give some examples of the current problems which are being attacked cooperatively by engineers and physicists, I shall use the two fundamental aeronautical examples just referred to, namely aircraft structures and hydro- or better aerodynamics. The latter deals with the interaction between the external surface of an aircraft and the fluid medium, air, which surrounds it, while the former is concerned with the transmission of the air forces from the surfaces where they arise to the useful load or other concentrated weights which are carried.

Turning first to the structural problem, there are three characteristic requirements for an airplane which together distinguish it from any of the conventional structures of civil engineering. First, there is the tremendous importance of lightness which puts a premium on careful design and exact analysis, and makes it not only desirable, but actually essential, to employ all possible refinements in structural calculations. It is therefore economically sound for an airplane manufacturer to employ highly specialized and scientific analysts, while the work of such men could not be justified for most conventional engineering problems. Second is the requirement that an airplane have a smooth, strong external surface for the air pressures to act on. This suggests the desirability of giving the external surface two functions to perform simultaneously: carrying the external pressures and serving as an integral portion of the major structure. This idea has led to the virtual abandonment (at least in many of the most modern aircraft) of the older types of design which had a sort of skeleton basic structure with fabric external surfaces. They have been replaced by the so-called "monocoque" or "stressed skin" type of design in which the surface is made of thin metal and carries a considerable fraction of the major structural loads. The third characteristic feature is that aircraft structures in general transmit relatively small loads over very large distances. This implies that the structural members will usually have lengths which are very large compared to their transverse dimensions.

The use of stressed skin construction means that aircraft structures are highly redundant or indeterminate. The classical methods of stress analysis based essentially on equilibrium considerations completely fail for such cases, and recourse must be had to the complicated and highly mathematical theory of elasticity. Furthermore, the use of thin skins and the great slenderness of structural members result in

tendencies towardsinstability or local buckling. Neither of these problems originated with air-planes, but the advent of the airplane did impose such heavy penalties on their solution by the old method of adding enough material to take care of "factors of ignorance" that their study was given a remarkable impetus. Some of the results of this study which have been widely applied in practice are so unorthodox from the point of view of the older structural tradition as to be very startling even to the layman. Among the most remarkable are those which are connected with questions of instability or buckling. For long compression members like struts, beam flanges, etc., the problem has been attacked in two manners. First, by designing the structure so that such members have lateral support at frequent intervals, the ratio of length to transverse dimensions is kept fairly small between supports and the tendency to buckle is much reduced. Second, by very carefully worked out cross-sectional shapes, a given amount of material can be so distributed as to give the largest possible moment of inertia, which means that the effective width of the member is increased with a consequent decrease in length-width ratio and an increased resistance to buckling. Figs. 1 and 2 give a few examples of cross sections which have actually been used for metal compression members, and show clearly the extreme lengths to which this type of development has gone.

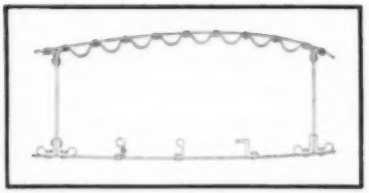


FIG. 1. Cross section of duralumin monocoque wing. Various shaped compression members are shown attached to the bottom skin.

When we come to the question of the buckling of sheets used for wing or fuselage covering in stressed skin construction, the problem is still more difficult. In order to prevent all buckling of these sheets, the thickness would have to be so great or they would have to be supported by so many stiffening elements that the weight would be completely prohibitive. Some years ago this seemed to be fatal to the use of such covering sheets, since in classical structural design buckling was usually considered as practically synonymous with failure of the structure. That this time-honored tradition is not universally true can be seen very easily. If a piece of paper, say ordinary typewriter paper, is held with two hands on opposite edges and a shear is applied, the paper promptly wrinkles or buckles. However, it is still carrying a very respectable load and when the load is removed, if too much has not been applied, the paper returns to exactly its original condition with no sign of damage. The same type of behavior may be seen with thin metal sheet, although the testing apparatus must be a little more complicated. A young German scientist and engineer, Dr. Wagner, studied the question very carefully a few years ago and took the bold step of conceiving structures which were allowed and even designed to develop waves under load. He furthermore gave

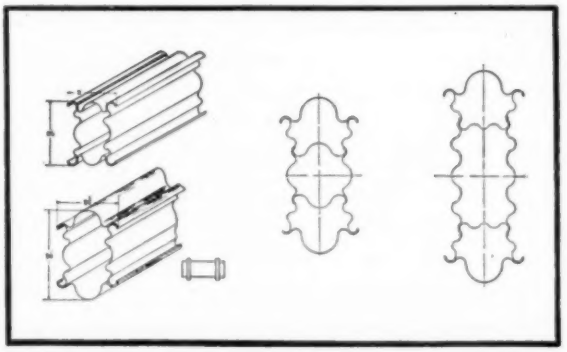


FIG. 2. Compression members built of stainless steel. The extent to which elaborate forming has been carried in order to obtain stiffness is very apparent.

practical design formulae for such thin sheet structures in which the sheet buckled or, in modern terminology, "went into the wave state" under load. Many of the latest and most modern airplanes are designed on this principle and use the Wagner formulae. The usual method of design at present is based on the fact that during practically all of the time an airplane is flying, the loads in the members are those arising essentially from a static equilibrium between the plane's weight and the lift of the air. However, in violent maneuvers or in flying through gusty weather or storms, the loads may attain values many times those occurring in smooth level flight. The structure is designed so as not to buckle during normal flight, but so that the sheet goes into the wave state during the brief instants when the loads are higher than normal. The

phenomenon can be very clearly observed in most of the modern, low-wing transport planes. If one catches the light correctly, he can plainly see the waves appear and disappear on the wing upper surface as the plane flies in even moderately "bumpy" air. If any of you have ever made such observations with some slight feeling of trepidation, I hope that the simple explanation just given may prevent any recurrence of your uneasiness.

In concluding this very brief discussion of structural problems, I should like to present two figures showing rather extreme cases of "wave formation" as observed in the laboratory. The first (Fig. 3) shows a thin flat sheet of duralumin tested under compression in the laboratory. The waves are very apparent, and all but the one in the lower left-hand corner disappeared when the load was removed. The second, (Fig. 4) shows a complete monocoque wing under static test. The photograph on the right shows the wing with no load, that on the left exhibits the same portion of the wing when the latter was loaded to the full extent for which it was designed. The waves are not nearly so violent as was the case in the

FIG. 3. Flat duralumin panel under compression. All the waves except that in the lower left hand corner disappeared when the load was removed.

simple panel tested in the laboratory, but they are none the less very plainly visible. When the load was removed, all traces of the waves vanished entirely. It is waves of this type to which reference was made in the paragraph above, and which may often be seen in flight.

Turning now to the field of aerodynamics, let us first review briefly the historical development in which it will appear that scientists' efforts have had a profound influence on engineering practice. The first fundamental problem of heavier-than-air flight is obviously that of obtaining support or lift from the air by the dynamic effects produced when a solid is moved through a fluid. One of the earliest scientific analyses of this question was made by the great physicist Isaac Newton, who developed a theoretical formula for the lift of a flat plate. This formula predicted that the lift should be proportional to the square of the angle of inclination of the plate to the relative wind, and from it could readily be deduced the conclusion that in order to obtain reasonable lifts the angle of inclination would have to be so large that an enormous amount of power would be required

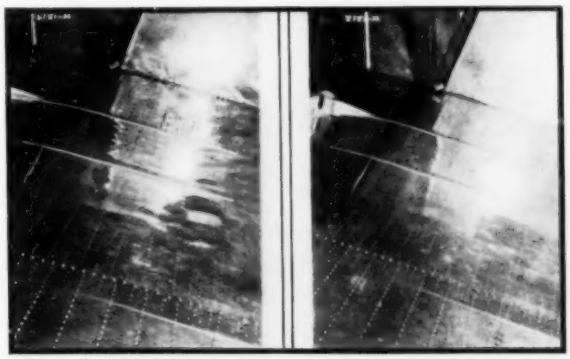


FIG. 4. Wing upper surface of a modern transport under test with no load and full design load. The waves which appear in the left hand picture for the loaded wing completely vanished when the load was removed.

to drive the plate through the air fast enough to retain its lift. This theory, which was later found to be based on false, or at least unreal, assumptions, had most unfortunate consequences, since for centuries it was believed to demonstrate the impossibility of human, heavier-than-air flight. For this reason, most scientists were dissuaded from investigating the problem further, as were also many practical inventors and experimental investigators.

Fortunately, there remained a few hardy souls in both camps who refused to be daunted in their investigations of the problem of lift by this gloomy prognosis. Near the beginning of the present century, the practical experimenters actually built machines which flew, and the theoretical researchers discovered the true laws of lift, showing that the lift of a plate was proportional to the angle of inclination rather than to the square of the angle. This result was found to be in agreement with experiment, so that the physicist was fortunately extricated just in time

from the embarrassing position in which the lowly inventor could say "You state that flight is impossible, please come and look at my machine which flies." It must be confessed, however, that for the next few years, the men engaged in developing the new flying machines showed little confidence in scientists, and in fact pretty consistently ignored their activities.

As data were slowly and laboriously assembled by crude trial and error methods, it appeared that one factor was of great importance. This was the lateral extension of the supporting wings, or, as it is now called, the wing span. Not only did this affect the relation between lift and wing inclination to some extent, but it appeared to have a very large influence on the resistance to forward motion, or the drag, as it is now termed. Since the drag must be balanced by the thrust given by engine power, and the power of the early engines was very limited, the extreme importance of this relation between span and drag was soon realized. As experimental methods of testing wings were developed, much attention was directed to this problem, and an immense amount of data was collected. However, the experiments were often inexact and many other factors influenced the drag in ways which were not understood, so that instead of clarifying the question, the mass of empirical observations actually led to a confusion which amounted almost to chaos. Engineers knew that span was an important element in airplane design but were quite in the dark as to the magnitude of its effects, or even in some cases as to its qualitative influence.

Finally in 1911, Prandtl, one of the members of Klein's group, and by that time director of the famous Goettingen aeronautical laboratory, gave a purely theoretical analysis of the problem which led to very simple formulae giving the effect of span on drag and on the relation between lift and wing inclination. These formulae were carefully checked experimentally at Goettingen and elsewhere and found to be remarkably accurate over the range of wing proportions used in normal airplanes. At once the earlier confusion began to disappear, and the mass of experimental material, when interpreted in the light of the theory, fell into logical and simple order. This result was so striking that engineers were rapidly forced to study the theoretical background of the formulae and to come into close contact with the physicist's point of view underlying them. The breach between the engineer and the scientist was closed with astonishing rapidity, never more to reappear in the field of aerodynamics. At the present time, almost every calculation of airplane performance and every serious layout of a new airplane uses the Prandtl formulae explicitly and the basic physical conceptions more or less intuitively. Indeed, our whole picture of the lift and drag of wings is so based on the conceptions worked out by the mathematician and physicist that it is hard for us to see how the early practical pioneers, without this picture, succeeded as well as they did.

Unfortunately, the so-called "induced drag" calculated by Prandtl and arising from the finite span of airplane wings is not the only type of resistance which the laws of nature furnish to plague the aeronautical engineer. The other types are roughly independent of span and can be divided into two categories. One is called pressure drag and is the resultant of all the normal pressures acting over the surface of a body moving through a fluid. It has large values for so-called bluff bodies and has been discussed by many physicists, starting with Helmholtz, Kirchoff and Rayleigh. The true physical mechanism and approximately correct analytical formulae were finally given by von Kármán, another member of Klein's group, in 1911. Fortunately, these formulae should not be necessary to the designer, at least if he is a good one. For by proper streamlining this pressure drag can be completely eliminated. If pressure drag exists on an airplane, it indicates either that the designer was not sufficiently able or that he had to compromise with some nonaerodynamic, practical or perhaps military requirements. The enormous jump in commercial and military airplane performance which has occurred in the last few years is largely due to the recognition and active use of this principle. In this connection it may not be amiss to digress a moment to remark on this beautiful example of the connection between physical laws and aesthetics. In almost no field is this more strikingly apparent than in that of hydrodynamics. A body which is really properly streamlined in the sense of having a shape which

eliminates pressure drag, is almost invariably an aesthetically satisfying object. This is certainly exemplified by a comparison of the sleek modern transport with its ungainly predecessor of ten years ago, covered with excressences, struts, wires, and corners, each of which added its full quota of pressure drag and subtracted its share of aesthetic value.

The one remaining form of drag under consideration cannot, unfortunately, be exorcised by the application of any such simple phrase or conception as "streamlining." Indeed, as pressure drag was eliminated and induced drag controlled by proper application of the Prandtl formulae, this remaining component of drag became of primary importance. This component has as its origin that bane of the engineer, namely "friction," and as such can presumably be controlled but never eliminated. It is clearly essential that its nature be thoroughly understood in all its details, if attempts at its control or reduction are to have any large success. Here again is a great opportunity for the applied physicist and one which he has eagerly seized. The problem remains far from completely solved, but great advances have already been made. Once more it was Prandtl who took the first step in his classical "Boundary Layer" paper in 1904. In this paper he showed that in practically all questions of aeronautical importance, the influence of friction was confined to a thin layer of air close to the surface of a body moving through the atmosphere. This so-called boundary layer has been vigorously investigated ever since, until now its essential features are understood for cases in which the flow remains well-behaved and does not develop an eddying or so-called turbulent behavior. I shall later return to a discussion of this vital question of turbulence, and for the present shall only point out the fact that, largely as a result of the scientists' efforts, fairly satisfactory and simple formulae are available to the engineer for the calculation of friction drag, and the major factors affecting it are moderately well understood.

Another branch of aerodynamics whose development owes much to the mathematician and physicist is that which is concerned with the motion of an airplane considered as a rigid body under the influence of the external forces and moments exerted by the air. The problem of first importance here is that of stability, i.e., assuming the airplane flying steadily in equilibrium, what happens when it is slightly disturbed, say by a gust or by a movement of the controls? The mathematical formulation of this problem was given by Bryan, an English scientist, in 1911, and the problem has been worked on ever since. As might be expected from its nature, it involves a rather extensive mathematical technique which does not lend itself to the "handbook" type of simplification. Indeed, until very recently it was necessary to go entirely through the tedious and lengthy numerical calculations for every airplane investigated. As a result, most practical engineers more or less ignored this question of "dynamic stability," as it is called, until they received the pilots' flight test report, after which unfortunate stability characteristics were improved or eliminated by cut and try empirical methods. Within the last few years, this situation has been much changed. Several research investigators drew up large groups of charts showing stability characteristics for families of typical airplanes. By interpolation, the designer can now estimate the stability characteristics of a proposed new airplane of conventional type without going through the laborious calculations which were formerly necessary. Also, the great increase of interest in flying qualities in connection with the new large passenger transports has made it essential for engineers to study seriously the dynamical behavior of airplanes under all kinds of conditions. The result is that in this field, as in the others mentioned above, the gap between the applied physicist or mathematician and the practical engineer is rapidly being closed.

After this brief review, of a more or less historical nature, I should like to conclude my paper with a short discussion of the most difficult and fundamental problem in contemporary aerodynamics. The problem of turbulence, as it is usually called, has occupied many of the best mathematical and physical brains in the world for the last fifty years, and it still remains essentially unsolved, although great advances have been made. The great British physicist, Osborne Reynolds, first studied the problem of turbulence systematically in 1883, and his original experiment still furnishes what is probably the best in-

troduction to the subject. In this experiment he employed a long glass tube through which water flowed from a large reservoir. At the entrance to the tube he placed a small orifice in the reservoir, from which orifice ink or colored fluid was allowed to flow. The ink was carried by the water and

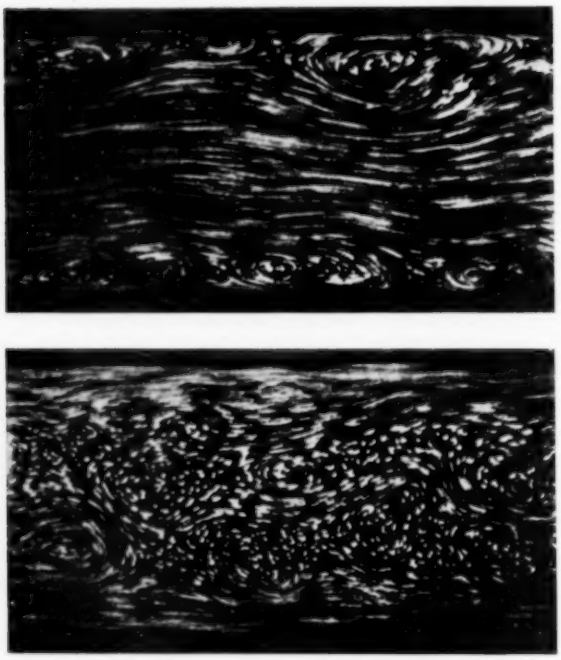


FIG. 5. Photographs of turbulent flow through an open channel taken with a moving camera. In the upper photograph the camera was moved with approximately the velocity of the fluid layers near the wall. The lower photograph shows the same flow with the camera moved at approximately the velocity of the fluid in the center of the channel.

formed a fine colored ribbon or band extending down the glass tube. The experiment consisted in observing the color band as the velocity of flow through the tube was gradually increased. At low velocities the band remained sharp and distinct for its entire length and was everywhere straight and parallel to the tube axis. Reynolds concluded that the fluid particles traveled in practically straight paths so that the motion was essentially that of an infinite number of very thin lamina sliding smoothly over one another. This type of motion is, as a consequence, called laminar. As the speed was increased, Reynolds found that the laminary regime prevailed until a definite "critical speed" was reached. At this speed the color band first became wavy and then broke up entirely, the color becoming diffused throughout the entire tube. Reynolds' interpretation was that in this new flow regime the particles of fluid moved not in straight lines but along irregular and sinuous paths so that there was a continuous mixing between the various layers of fluid. Reynolds at first described this state of motion as "sinuous flow," but it is now almost universally called "turbulent flow" because of the irregular and turbulent paths which the particles of fluid follow.

It is now known that Reynolds' observations constitute a special case of an almost universal phenomenon. For practically every type of fluid motion the flow is laminar at low speeds and becomes turbluent at a definite critical speed, above which it always remains turbulent. Reynolds found that it was not actually the speed which was critical, but the dimensionless ratio VL/ν where V is a characteristic velocity of the flow, L a characteristic length of the system, and ν the coefficient of kinematic viscosity of the fluid. This ratio is now called the Reynolds Number and a critical Reynolds Number, at which the transition occurs from the laminar to the turbulent regime, exists for almost every type of fluid motion. It appears that in practically all cases of technical importance, both hydraulic and aerodynamic, the range of Reynolds Numbers encountered lies far above the critical one, so that a knowledge of the nature of turbulent flows is of extreme practical importance.

Unfortunately, the importance of this question appears to be exceeded only by its difficulty. Fig. 5 will give some idea of the complexity of a turbulent flow. The slide refers to flow through an open channel, and the flow has been made visible by scattering aluminum powder on the surface. The photographs are taken with a camera which can be moved along the channel with arbitrary speed. Aluminum particles moving with the same speed as the camera appear as dots, those moving relative to the camera are recorded as lines whose length is proportional to the relative velocity. The photographs show how highly eddying and disturbed a turbulent flow actually is, and also bring out how completely different it appears when referred to two different frames of reference.

Photographs like the above are useful in getting a qualitative picture of turbulence, but for quantitative measurements more refined technique is required. One type of apparatus which has been widely used for this purpose is the so-called hot wire anemometer which consists essentially of a very fine heated wire placed in an air-stream. The air velocity is measured by the amount of cooling which it causes to the wire, and the latter is so extremely fine that it follows very rapid velocity fluctuations. Oscillograph records from such a hot wire in a turbulent air-stream are reproduced in Fig. 6. Time appears as abscissa while instantaneous velocity is the ordinate. The very rapid and extremely irregular nature of the fluctuations makes such a record a very depressing curve to try and analyze. Indeed, all attempts to find any periodicity or regularity in such oscillograms have completely failed, so that the phenomenon appears to be an essentially irregular or statistical one.

Having gotten some idea of the nature of turbulent flows, let us see what a few of their technically important effects are. The essential feature of any turbulent flow is the active mixing which goes on between neighboring layers due to the irregular and fluctuating motion of the fluid particles. This implies that any diffusion process, where some property is carried by the fluid particles, is very much accelerated in a turbulent flow. The diffusion of heat furnishes a very clear example of this phenomenon. Fig. 7 shows schematically the conditions which exist behind a heated wire in a laminar and in a turbulent flow. The ordinates of the contours give temperature and the dotted lines, which are drawn through the points of half-maximum temperature, give an idea of how the wake spreads out in going downstream. Neither figure is drawn to scale and the wake in the laminar case is relatively very much narrower than shown. The much more rapid diffusion of heat in the turbulent flow is a characteristic which is by no means limited to the simple case shown in the figure. It is of great technical importance in all types of heat transfer problem. In aeronautics it is vitally important in connection with the cooling of radiators and air-cooled engines, and a considerable proportion of the recent improvement in aircraft engine performance must be credited to our increased knowledge of the phenomenon of heat transfer in turbulent flows.

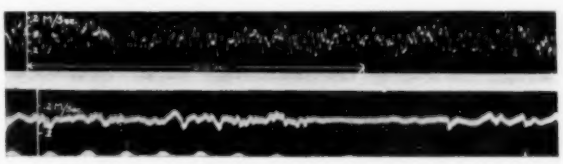


FIG. 6. Oscillograph record of velocity fluctuations in a turbulent air-stream, obtained with a hot-wire anemometer.

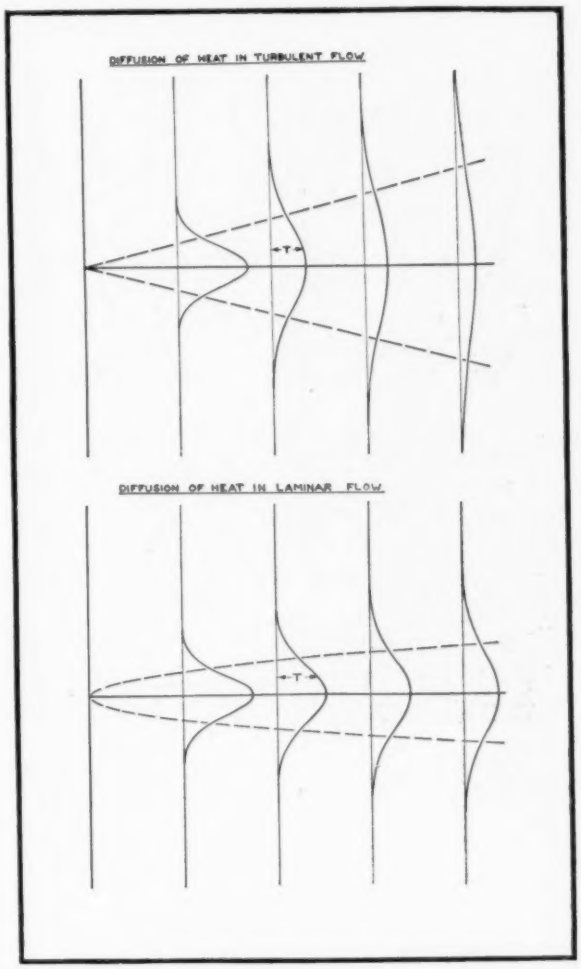


FIG. 7. The wake behind a heated wire in laminar and turbulent flows. The profiles give temperature distribution as a function of position. The more rapid diffusion in the turbulent flow is shown schematically. The laminar wake is much narrower, relative to the turbulent one, than the diagram indicates.

A second type of transfer problem which is of even wider importance is the transfer of momentum in fluids. The friction which exists between neighboring fluid layers can be considered as

arising from the momentum transported across the sheet between them. In laminar flow the momentum is that of the individual molecules which cross from layer to layer because of their molecular agitation motion corresponding to thermal energy. In turbulent flow the momentum is that of large fluid masses which move transversely to the mean flow, corresponding to the turbulent fluctuations. The effect is to increase enormously the friction, or better the apparent viscosity, between neighboring layers. At a solid surface the turbulent fluctuations must vanish, and they increase in intensity as the distance from the surface increases. The result is that in a turbulent flow the apparent viscosity increases rapidly as the distance from a solid boundary increases. The effect on the flow through a channel is shown in Fig. 8. The dotted curves show the outer edges of the two boundary layers within which the effects of friction are important. At a distance L_1 from the entrance, the two boundary layers join and at a large distance L_2, the mean conditions are steady so that all velocity profiles further downstream are identical. In the laminar case the final velocity profile has a parabolic shape as shown in the figure. In the turbulent case the profile is much flatter, since the very high apparent viscosity far from the wall means that the fast moving layers in the center drag their neighbors with them. Near the wall this effect is much reduced and the velocity gradient becomes very large. The most important practical result of this change in velocity profile is the fact that the frictional resistance at the wall, which is proportional to the slope of the velocity profile at the boundary, is very much larger in the turbulent than in the laminar case. This is a general result in channels, tubes, boundary layers, etc., and the high frictional resistance of tubulent flows is obviously of the greatest practical importance. Other differences between the two types of flow, which appear in the figure are the more rapid increase of the boundary layer thickness, and the smaller values of L_1 and L_2 for the turbulent case. Both of these effects are actually much larger than they could be shown on the figure, the lengths L_1 and L_2 being tens or hundreds of times as large for laminar flows as for turbulent.

There are many other phases of turbulent flow which are of great practical importance, but the two examples cited will perhaps indicate the essential character of the problem. When we come to the solution of this fundamental problem of turbulence, we are somewhat depressed at the meager extent of our past success, but should be much stimulated by the magnitude of the field which remains to be explored. Prandtl and von Kármán have made important contributions based on dimensional analysis and the latter has developed practical and widely-used formulae for skin friction based on principles of mechanical similitude, but the fundamental mechanism of turbulence remains shrouded in mystery. G. I. Taylor in England has recently published a very important series of papers

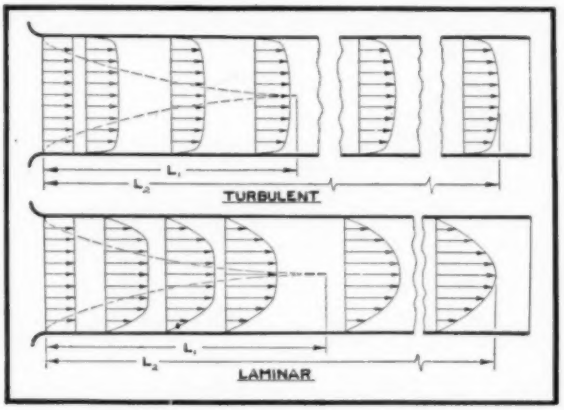


FIG. 8. Velocity distributions in the entrance region of a channel for laminar and turbulent flows.

treating the problem statistically, and it appears now that this may prove to be the tool which will finally build up a consistent picture and theory. At any rate, the problem now offers one of the most fascinating fields of research for the physicist and one which, in spite of its abstract and difficult nature, is in immediate and intimate contact with the practical concerns of the engineer.

I hope that in the course of the above remarks, I may have given a little insight into the essential character of the cooperation between physicist and engineer in the field of aeronautics, and that the final example may indicate how important is the "unfinished business" confronting the applied physicist in this field.

Samuel Montgomery Kintner

THAT public recognition in science comes to those not occupying academic positions is well illustrated in the life of the late Dr. Samuel Montgomery Kintner, Vice President in Charge of Engineering of the Westinghouse Electric & Manufacturing Company. Scientist and engineer, possessing a brilliant mind, radiating health, enthusiasm and cheerfulness, and endowed with a keen sense of humor, he rose to a high position in the scientific and business world.

Born in New Albany, Indiana, December 11, 1871, he there received his early education in the public schools and in a business college. After a year's clerical work with the Pennsylvania Railroad Company in Louisville, Kentucky, he entered Purdue University in the autumn of 1890, graduating from its electrical engineering course in 1894.

Dr. Kintner's record at Purdue University has always been remembered by many of his fellow students and by members of the faculty. His scholastic standing; his participation in athletics, as a member of Purdue's famous football team in the early 90's; as one of the battery of its baseball team; as a participant in various track events; as president of the Purdue Athletic Association—only gift of the entire student body; his social life; his general popularity, all forecast the successful life that was to follow.

For one year after graduation, young Kintner was manager of the exchange of the American Bell Telephone Company at Frankfort, Indiana. Recognizing the limitations he resigned in September, 1895 to accept a teaching position in the electrical engineering department of the Western University of Pennsylvania, in Allegheny, Pennsylvania, now the University of Pittsburgh.

Dr. Kintner's association with the University of Western Pennsylvania came about through his acquaintanceship with Reginald A. Fessenden, head of the electrical engineering department of that institution, who had held a similar position at Purdue University when Dr. Kintner was an undergraduate. He laughingly said in later days that Professor Fessenden's need for an experienced football player was the real reason for his employment. In his characteristic way he sized up the requirements of his new position, studied constantly, and in a short time adapted himself to his new environment.

Those associations with Professor Fessenden had a marked effect upon Dr. Kintner's entire life. Professor Fessenden was at that time deeply interested in wireless telegraphy and radio transmission and held important inventions in this field. Dr. Kintner not only assisted Professor Fessenden in his teaching courses, but aided him in his research work in connection with wireless and radio transmission. Various scientists who had been engaged in studies of electromagnetic waves had developed improvements consisting largely in the detecting means used to intercept and indicate the existence of such waves. Hertz employed, as his detecting and indicating means,

CHRONOLOGY OF DR. KINTNER'S LIFE

1871	Born at New Albany, Indiana
1894	Graduated from Purdue in E.E.
1895	Bell Telephone Company
1895–'03	University of Pittsburgh
1903–'07	Research, Westinghouse
1911–'20	General Manager, President, National Electric Signaling Company
1920	Director of Research, Westinghouse
1930	Honorary Sc.D., U. of Pittsburgh
1931	Vice President, Westinghouse
1936	Died at Pittsburgh

a minute spark gap in a single turn loop of wire. The most important of these improvements resulted from the work of Munck, Branley and Lodge. They resulted in the production of a device named by Lodge, a "coherer," which was very much more sensitive than those previously used. Said Dr. Kintner: "To Professor Fessenden, more than any other worker in radio, either in America or abroad, belongs the credit for devising methods and apparatus that have had the greatest influence in developing the art as we know it today. He was one, if not the first, of those to realize that the coherer, instead of being the essential element of a radio system, was the worst influence existing and was seriously hampering the advance of the art . . . . While others sought to improve the coherer, Professor Fessenden searched for entirely new detectors operating on different principles . . . . He was successful in his search for such a detector and discovered several that met the requirements of his specification. This radical departure of Professor Fessenden from the beaten path of his predecessors was of the greatest importance and had a wonderful influence, not only on the trend in development of the art, but also on the part that he played in it, and accounts, in a large measure, for the several basic inventions made by him which completely revolutionized the art.

"At least two other basic inventions of Professor Fessenden were the direct result of his use of his detectors. These were (a) the employment of waves generated continuously as from a source of high frequency alternating currents and (b) the beats method of reception named by Professor Fessenden the 'Heterodyne Method.'"

Today every long distance radio transmitting station of importance employs waves generated continuously, while the heterodyne method is that preferred for radio telegraph reception throughout the world.

In the summer of 1903, Dr. Kintner accepted a position in the research department of the Westinghouse Electric & Manufacturing Company, East Pittsburgh, Pennsylvania, where for four years he devoted his attention to high tension phenomena, alternating current electrolysis, telephone interference, motor flashing and other problems. His success in these lines resulted in his transfer in 1907 to the engineering department in charge of Railway Motor Design, where he assisted the late Benjamin Garver Lamme, chief engineer of his company in the design of motors used in the New Haven Railway, the St. Clair Tunnel and other important alternating current railway installations.

Never having relinquished his interest in the radio field, Dr. Kintner in 1911 left the employ of the Westinghouse Electric & Manufacturing Company to become General Manager of the National Electric Signaling Company, Pittsburgh, Pennsylvania, then engaged in developing commercially the radio inventions of Professor Fessenden. He soon rose to the rank of vice president and then president. It was during this time that the heterodyne system was developed to practically commercial form, that the Arlington Radio Station of the Navy Department was installed, and important installations for the United Fruit Company and other organizations were made under Dr. Kintner's general direction.

During the World War, the National Electric Signaling Company was engaged in the manufacture of radio equipment for the U. S. Navy and

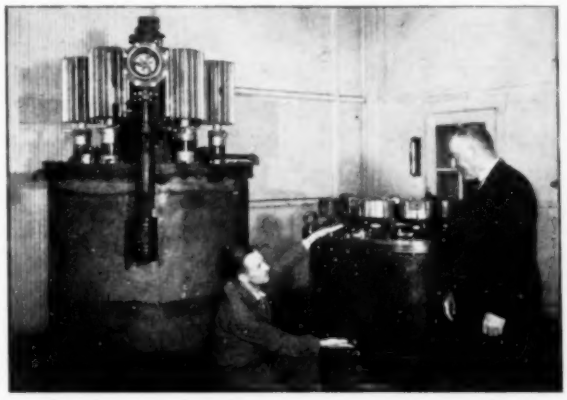
Dr. Kintner at the controls of his tractor which was used for cultivating on his summer farm.

the Signal Corps of the Army. During this period and later, Dr. Kintner gained a wide experience in patent litigations connected with suits for infringement by or against his company, as a result of which he became widely recognized as an expert witness in such cases. Many times he was recommended by all parties to a suit for the clear and simple manner in which he explained and

clarified highly technical phases of his subject so that they were understood by the layman, and for the utter fairness of his testimony.

After the close of the World War, Dr. Kintner negotiated the sale of the patent rights of the National Electric Signaling Company to the Westinghouse Electric & Manufacturing Company and himself reentered its employ. Here the world's first commercial broadcasting station was soon established and rapid progress was made in the development of the entire radio industry. It was soon appreciated that his abilities could be used still more effectively and he was made manager of the research laboratories. This proved to be a very happy choice since, on account of Dr. Kintner's breadth of engineering knowledge, his ingenuity, enthusiasm and executive ability, the company management had great confidence in his plans for the future and gave its research interest ample financial backing which was followed by many new and important accomplishments.

Expanding buildings, personnel and facilities required a revamping of his research organization. Dr. Kintner believed in carrying on research work as well as design and manufacturing facilities through a definite organization structure. Very soon, therefore, he organized the laboratory personnel into physics, metallurgical, chemical and general sections. A few months later, a mechanics section was formed. In the following year, an insulation section was organized. In the conducting of the work, Dr. Kintner was very careful to follow organization procedure and issued his instructions through his section engineers, except for special projects in which he was especially interested and in those activities where he dealt personally with the individual engineer, he was very careful to keep his section engineer informed regarding any instructions which he might issue to their men. Section engineers were given complete control of their budgets and subordinates and were held responsible for the results. If a section engineer were guilty of an error of judgment or action, which did not altogether agree with Dr. Kintner's ideas, he would still be backed to the limit in whatever action he had taken, although he might receive a private lecture on his shortcomings.



Dr. Kintner inspecting one of the recently developed sectionalized rectifiers which is only ¾ the size of its predecessor (shown on the left) for the same output.

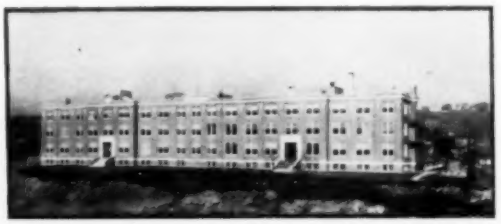
As still greater facilities became available, a modified form of organization seemed desirable to Dr. Kintner. The sections were changed to divisions, each under the direction of a division manager, and new magnetic and development divisions were added. The larger divisions were in turn divided into sections, each under a section engineer. Eminent scientists were added to the staff as consultants.

Dr. Kintner took the keenest interest in the personnel of his laboratories and always knew every individual by name, even the newest recruits, and was well acquainted with each man's strong and weak characteristics. Whenever a man showed promise or accomplished an outstanding piece of research, he was promptly rewarded. To make sure that he had not misjudged anyone or overlooked some promising man, Dr. Kintner at one time devised a questionnaire which was distributed through the organization on which ratings were entered for each man by his associates. These ratings were then checked by him and his own opinions were often revised as he realized a man's closest associates are often in a better position to know an individual's capabilities than anyone else.

Once a year, while he was in charge of the laboratories, Dr. Kintner invited his entire staff to visit his summer home in the country where he personally saw that everyone had a most enjoyable time, and himself became one of the boys. He encouraged social affairs as a method of promoting good feelings among the men and their families. At picnics, a soft-ball game was

often an important feature of the day. He would serve as pitcher for one side. He maintained his interest in athletics from his college days and in his later years was an ardent golfer and bowler. When the World Series was played in Pittsburgh, a few years ago, he attended the games and extended the same privilege to any member of his staff who wished to go. Then and in other years there was no curtailment of the use of the radio in the laboratories during World Series games.

He took a great interest in motion pictures from a research development standpoint and as an amateur photographer. He took many movies of the laboratory staff in their various recreational activities. Under his direction, Westinghouse Research was responsible for the development of the photophone equipment which was introduced commercially by RCA.



The present extensive facilities of Westinghouse's Research Laboratories, East Pittsburgh, Pennsylvania, are largely due to Dr. Kintner's steadfast belief in such activities.

Dr. Kintner searched constantly for new men to strengthen his technical staff and gathered men from many places. Under his direction the mechanics division soon came to be one of the strongest groups of men technically in this field in the United States and shortly acquired an international reputation.

Dr. Kintner early recognized the importance of carrying on development work in connection with television. He instituted a careful search for a suitable man to take over this activity, but at first could find no one who was interested. There happened to be a young Russian on his staff who had done some good work on photo-cell development which resulted in radical improvements in this device. Without much effort he was persuaded to start some work on television. Zworykin's name is now known all over the world to those who are at all acquainted with television. Dr. Kintner followed these investigations almost daily and television developed most rapidly. In a comparatively short time, a cathode-ray tube was developed and a receiver constructed and operated which is in all essentials the same as that which RCA has recently demonstrated. Just about the time that this receiver was constructed by Zworykin, it was decided to transfer radio receiving and television to the RCA. Zworykin, with his apparatus and some of his associates, went with the activity and Dr. Kintner's participation in this research ceased. It is a typical example of what his foresight and enthusiasm were able to accomplish.

Another example is that of the development of the alloy Konal. It was found in the manufacture of radio tubes that Westinghouse was using many thousands of dollars worth of platinum a week and in addition to the cost, there was danger that the available platinum supply of the world would be exhausted. Appreciating these conditions, Dr. Kintner characteristically immediately sought a solution. His first move was to choose a bright young man then on his staff and to put the problem up to him. By daily contact and suggestions, this man's enthusiasm and activities were kept at a high pitch by Dr. Kintner until in an astonishingly short time, there appeared a new alloy of nickel-cobalt-ferrotitanium which had a higher strength at high temperatures than platinum, thus making filament construction and support easier. Unexpectedly this new alloy was found to give better thermionic emission than platinum. The cost, of course, was but a very small percentage of that of platinum. Konal still stands in a class by itself as a desirable alloy for radio and industrial tube filaments.

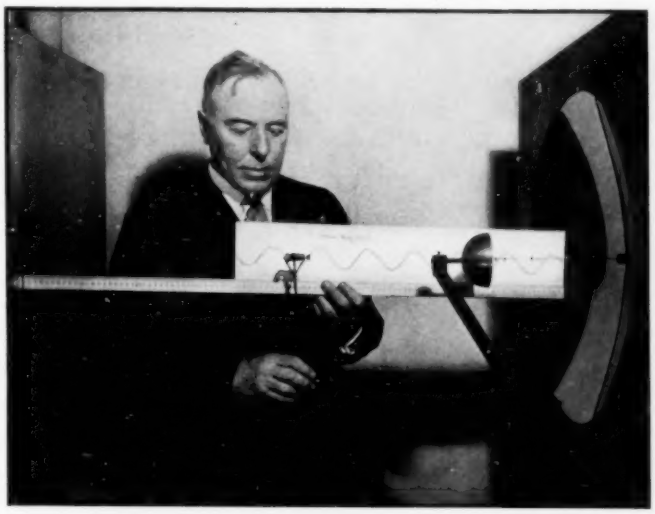
Radio tube development started in the Westinghouse laboratories shortly before Dr. Kintner took charge. He took a keen interest in these developments and followed them very closely. His suggestions and judgment concerning the lines which should be actively followed were invaluable. The a.c. tubes, both with direct and indirect heated filaments, were carried through from their beginning to their commercial production under Dr. Kintner's supervision. Many transmitting tubes were also designed and constructed, including the AW-220, the largest

sealed-off tube ever used commercially in this country.

After the organization of the RCA, Dr. Kintner was active in all tube standardization and coordination developments with the General Electric Company, being chairman of the Radiotron Coordinating Committee. He enjoyed the confidence of all members of this committee because of his honesty and fairness as well as his judgment in technical and commercial features.

Dr. Kintner was insistent on quality in all phases of the radio field as against temporary commercial expediency and his influence in this direction was undoubtedly important in producing better radio reception. He strongly urged the desirability of more powerful broadcast stations in which his judgment appears to have been correct in view of past and present tendencies in that direction.

When the Westinghouse Electric & Manufacturing Company's radio activities were transferred to the RCA, Dr. Kintner immediately started his tube men into an increased activity in the industrial tube field. This soon resulted in many new and valuable tubes which found a wide variety of industrial uses. This included vacuum, gas-filled and photo tubes. In this manner the personnel; technique and equipment which were accumulated for radio research were turned to good advantage in an allied and important field.



Always tremendously interested in electronics, Dr. Kintner was particulary close to his research workers' efforts in the field of ultrashort waves, including beam radio with 9 cm waves.

Dr. Kintner's personality was an interesting one. His ability to absorb quickly the essentials of a new idea or plan was exceptional and due to his retentive memory important things were not forgotten. If an individual was careless or deceptive, he was likely to be confronted weeks or even months later and called to account when the true facts came to light. His ability to remember names and faces as well as facts was a great asset. On one occasion a group of half a dozen Japanese engineers came to the laboratory for a visit. They were introduced to him and seated themselves in his office for a chat before going through the building. Dr. Kintner later summoned one of his associates and introduced each of the visitors correctly without an instant's hesitation and included each person's title and afilliations; this is a feat few could accomplish with the handicap of the unfamiliar oriental names. His versatility is illustrated by the fact that while president of the National Electric Signaling Company he was also made general manager of the Pittsburgh Provision and Packing Company which was controlled by the same financial interests. This was no scientific undertaking and was a field in which he had no experience. He soon found his problems were human ones and in a short time the losing business was put on a profitable basis.

Dr. Kintner was not only a pleasing speaker but he made many valuable contributions to scientific and economic thought. His earlier papers of a strictly scientific character were usually presented before the American Institute of Electrical Engineers and the Engineers Society of Western Pennsylvania. His published articles also appear in various trade papers and scientific magazines.

He was a member of the American Physical Society, many engineering societies, the National Research Council, Sigma Xi and Sigma Chi fraternities (Purdue) and of various social clubs. At the age of 64, Dr. Kintner died, September 28, 1936, at his home in Pittsburgh.

HAROLD W. COPE

Accomplishments of the Industrial Physicist in the Glass Industry*

By E. C. SULLIVAN
Corning Glass Works, Corning, New York

If our experience is a fair criterion, the trained physicist will in future be more and more in demand as industry learns his worth and comes to realize that to his hands can best be entrusted some of its most vital problems.

TO a chemist organizing research for a glass manufacturer it soon becomes obvious that while there is plenty of work to be done in the chemical field his problems are, in great measure, problems which require training in physics. The useful properties of glass, with the single exception of its resistance to weathering and to other chemical attack, are physical, and many glass manufacturing problems lie in the domain of the physicist. So it comes about that although glass-making is classed as a chemical industry our own comparatively small development staff has about twice as many physicists as chemists. Also we have physicists outside the development organization, one of whom is manager of a sales division and another a vice president of the company.

The preparation which best fits a physicist for glass manufacture we believe to be training in the fundamentals of physics and in research, without specialization either in molecular physics at one extreme or in glass at the other. In our experience a man of proven ability to carry on high grade research is most likely to be successful in manufacturing, whether in the plant or in the development department.

In speaking of industrial physicists we tacitly distinguish between those physicists on the one hand who are called engineers, who build on the basis of experience, expressed in part in formulas and rules, and those on the other hand whom we call industrial physicists, who are more concerned with fundamental principles, who experiment to get new facts, or who build on the basis of less familiar phases of the science of physics. The engineer has long since established his value to industry. The value of the industrial physicist has been recognized more recently.

The industrial physicist in a glass factory, like the research man in any manufacturing line, acts as a consultant and has, furthermore, three specific functions: first, he keeps watchful eye on quality of product, second, he improves manufacturing methods, and third, he develops new products. These functions overlap and I shall make no serious effort to separate them in this discussion.

As a consultant he renders technical assistance in legal and patent matters and he advises on an endless variety of manufacturing problems.

When a glass building block is under consideration, for instance, the physicist is asked for information on transmission of light, heat, and sound, on condensation of moisture, on load resistance, wind resistance, fire resistance. He guides the design of a surface of pleasing appearance which will reflect the desired amount of incoming light and heat, which will diffuse sunlight and sufficiently obscure vision.

Or, with a window glass which absorbs radiant heat he is asked how much air-conditioning energy will be saved in summer. He is asked about the design of an endless variety of objects, from radio bulbs to lenses for airport beacons, from bottles to glass-melting tanks.

The continuous glass-melting tank is an inefficient, box-like structure which receives at one

* Presented at the meeting of the Founder Societies of the American Institute of Physics, October 29–31, 1936 in New York.

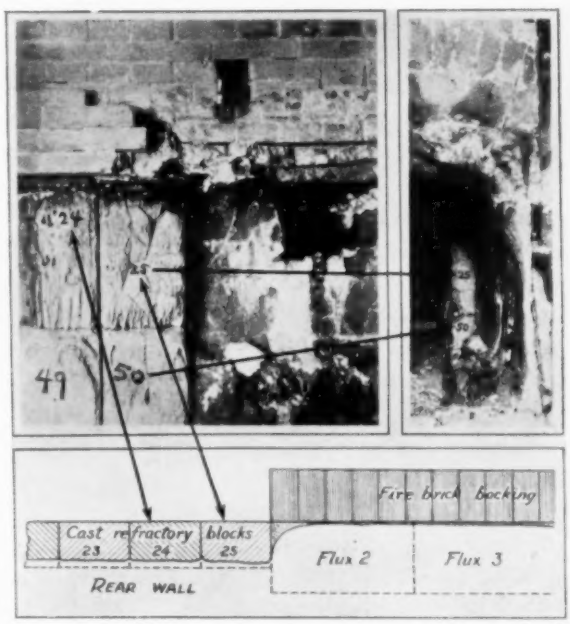
end raw materials and delivers at the other end molten glass. The physicist has traced out the course of convection currents in the glass which tend to mix partially melted material with that completely melted and thus slow up the melting operation, and has shown how proper temperature distribution over the glass surface helps in cutting down such mixing. If the glass is "specky" with gas bubbles which have not been expelled in the melting or if it is "stony" with bits of undissolved sand or refractories or crystallized material, the physicist is the consultant of last resort as to the remedy. Vital questions in glass melting obviously in the sphere of the physicist are the transparency of molten glass to radiation, radiating power of luminous and nonluminous flames, rate of expulsion of gas bubbles as related to size of bubbles, and viscosity of glass, and therefore to temperature.

Perhaps the most outstanding contribution of all time to the problem of glass refractories—a major difficulty of the industry—has been made by the physicist.

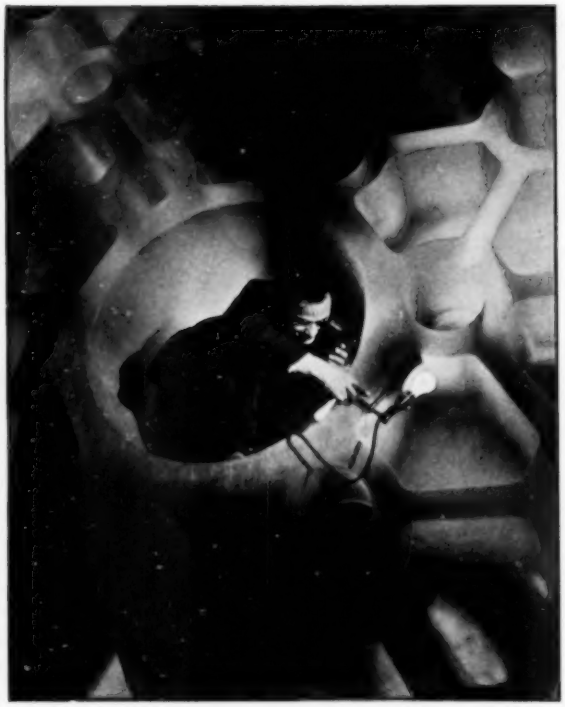
The glass-melting tank is built of blocks twelve inches thick up to two to three feet long and wide, put together very much as a child would build an open box of toy blocks. The rather porous clay blocks dissolve in the molten glass and where the attack is most severe, especially at the upper surface of the glass, it is only a matter of months before they are eaten down to a thin shell. Each year production would be interrupted for a period of weeks in order to cool down the tank and replace the old blocks. At times the molten glass would break through without warning, causing an unplanned shutdown which entirely upset manufacturing schedules.

Corundum, the crystalline form of alumina, and mullite, a crystalline compound of silica and alumina, were known to be resistant to glass attack, and the industrial physicist's idea of making tank blocks was to melt aluminum silicates in an electric furnace and to cast the melt into a mold, very much as a casting is poured in an iron foundry. The product is a dense, crystalline material two or three times as resistant to glass attack as the clay block and capable of withstanding higher temperatures without softening. It has prolonged the life of the tank, has greatly improved average glass quality by reducing streaks and striae which had been caused by the clay of the blocks, and has increased productive capacity through the more rapid melting of glass brought about by higher operating temperature. Melting cost and repair cost per ton of glass are lower and because more ware is obtained from a given number of tanks the required capital investment is less. The glass manufacturer has demonstrated his appreciation by steadily increased buying of the new block during the depression, and in 1935, barely seven years after the blocks were first offered to the public, of two hundred and fifty glass tanks in operation in the United States only twenty were entirely without the new material.

In standing guard over the quality of product the physicist sets standards, works out suitable rapid measurements, and supervises the carrying out of such measurements. The property to which a certain glass owes its characteristic usefulness, or the property which best serves as a check on the uniformity from one melt to another, is measured constantly.



Front and side view of the wall of the dog-house of a bulb glass tank of Corning Glass Works after fourteen months' run, showing the comparative corrosion of the "physicist's blocks," on the left, and clay blocks. Below: diagram of the construction of the dog-house wall. The dotted lines show the surface of each type of block at time of construction. The shaded area shows the portion of the blocks remaining at the end of the run.



The physicist examines the 200-inch telescope disk for strain.

By way of illustration, the electrical resistance of some glasses is quite sensitive to variation in composition. Now the wires which supply current to an electric lamp or radio tube are carried from outside through a short length of glass tubing, into which they are sealed. Unless the tubing has high electrical resistance the current will short-circuit through the glass from one leading-in wire to the other instead of passing through the filament. It is necessary to pay constant attention to the resistance of this so-called stem tubing.

The thermal expansion coefficient also must be watched in such stem tubing, for the glass must seal both to the wires leading in to the filament and to the enclosing bulb. This property of volume change with change in temperature is followed closely in other glasses as well, such as those for railroad lantern globes, laboratory ware, and baking dishes, where low expansion coefficient enables the article to live through violent temperature fluctuations without breaking.

The softening point determination, really a viscosity measurement, gives a good check on the uniformity of a given glass as melted from day to day and is important in itself in the case, for instance, of electric lamp bulbs which are worked in the lamp factory on automatic lamp-making machines requiring the glass to soften always to the same extent, in a fixed flame.

Other measurements cover a very wide range of the properties which make glass useful and the total runs into many thousand in the course of a year.

With all its good qualities glass will break, and it does happen that the user is inclined to lay the responsibility for breakage on the manufacturer. It is to the advantage of all concerned to get at the facts and here again the physicist steps in and from examination of the fracture, the condition of the surface, and the pattern in polarized light, tells us whether the article has had much use and whether it broke from a blow, from internal pressure, or from extreme heating or cooling, and frequently can suggest modifications in design or in the way the glass is used which will obviate future difficulty.

Perhaps the first instance of routine high temperature measurement on a large scale occurred some thirty years ago when a physicist installed platinum platinum-rhodium thermocouples in glass furnaces. In spite of the skill of the firemen in judging temperatures by the eye, actually the furnaces had fluctuated between heat on the one hand which softened and destroyed the pots in which the glass was being melted, and insufficient heat on the other hand to melt the glass properly. The temperature measuring equipment curtailed such losses and brought about immediately substantial increase in productivity of the furnaces.

The collaboration of the physicist with the chemist in glass composition development is illustrated in the glass for railroad lanterns to withstand rapid heating and cooling so that when the globe in a lighted lantern is chilled by rain or snow it does not crack and fail the trainman at a critical moment. The best glasses from this point of view are in general those which change volume least when heated or cooled.

Finding a suitable composition of low thermal expansion for such a purpose is not simply a matter of plotting a few points on a curve. There is no object in figuring out a composition of low

expansion if you can't melt the glass. Actually you must get the low expansion, or any desired outstanding characteristic, without serious departure otherwise from the usual properties of glass such as ease of melting, workability, freedom from devitrification, resistance to attack by water and other reagents, volatility of batch constituents, color, etc. Starting with a composition of known properties the approach to the desired glass therefore is by a series of short steps. At each point we must know what the change in composition has done to the other properties as well as to the expansion, and accurate measurement on the part of the physicist is sometimes required to show whether or not we are moving in the right direction. These and other physical measurements may become in large part routine but the devising of simple methods and the supervision of their carrying out, to give results that can be depended on, are far from routine.

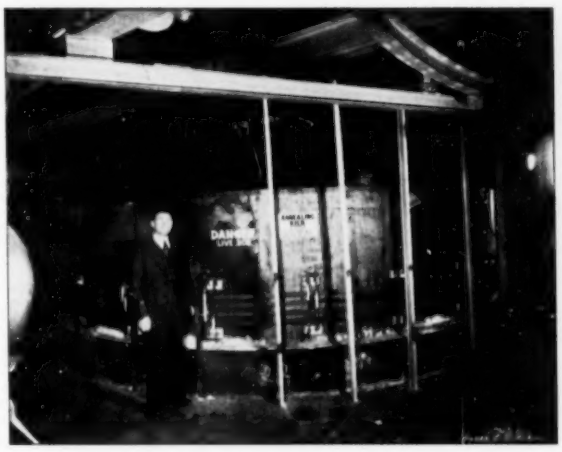
Produced in this country originally for railroad use, glasses of low expansion have found wider application in other fields. Chemical beakers, flasks and apparatus in general, equipment for chemical manufacture, dishes for household use in the kitchen and on the table, and miscellaneous ware constitute a business which scarcely could have been built up without the aid of the physicist.

The physicist was indispensable also in such developments as that of a glass for incandescent lamp bulbs which reproduced the properties of the lead glass formerly used and yet lent itself to tank melting and thus made possible modern continuous automatic bulb production which superseded intermittent melting and hand-working in pot furnaces.

The redesign of railway semaphore lenses and standardization of railway signal colors afford another example of the activity of the physicist in creating new products out of old. Each railroad had independently adopted a set of colors and no two reds or greems or yellows were exactly alike. An orange-yellow signal might be mistaken for red, or, more serious, a pale red might be mistaken for yellow. The situation was unsatisfactory from the standpoint both of railroad operation and of glass manufacture. The glasses now standard on the railroads of the United States, and some foreign countries, were chosen from two points of view: first, that the colors be as distinctive as possible, the red, for instance, well removed from yellow, the yellow neither too green nor too red; second, that the maximum percentage of light from the light source be transmitted. The physicist, furthermore, by redesigning the lens on the basis of the actual shape and size of the flame rather than on the basis of the theoretical point source of light doubled the distance at which the signal could be seen.

The manufacture of signal glasses is supervised by the physicist. A sample of each melt subjected to photometric measurement if not falling within prescribed limits, both as to color and as to transmission, causes rejection of the pot. After manufacture each piece of colored railway signal glass is inspected photometrically.

For glasses of special electrical properties the physicist has brought about large volume commercial applications. His measurements of dielectric strength have led to the marketing of glass insulators of both pin and suspension type for power lines, and he has demonstrated that they offer definite advantages. With his fellow physicist, the electrical engineer, he has improved



Dr. George V. McCauley, physicist in charge of disk making of the Corning Glass Works, beside the annealing kiln in which the 200-inch telescope mirror disk was subjected to a gradual reduction of temperature over a period of eleven months. The top section of the annealer is suspended by a forest of tie-rods carried by girders. The lower half is supported from the floor below. An insulating seal is provided by the white powder in the trough-like rim which can be seen surrounding the top of the lower portion.

insulator design to produce strength formerly unequalled. Similarly he has introduced for transcontinental telephone line insulation a glass having unusually low dielectric loss. Such glass finds application also in insulation for radio sending and receiving sets.

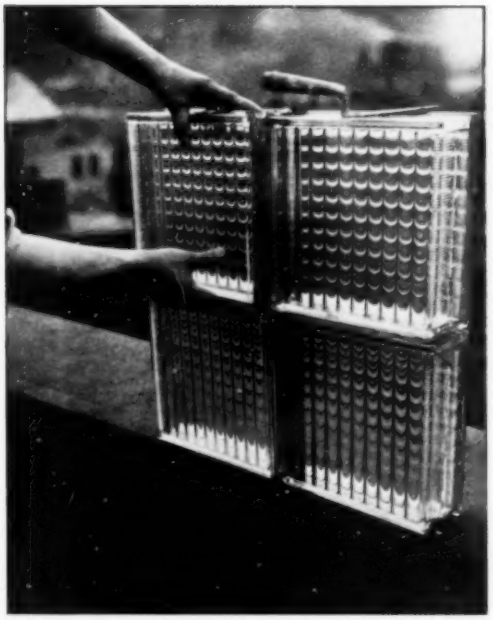
The physicist and chemist working closely together have evolved glasses aside from the railroad signal glasses already mentioned, for filtering out or for transmitting specific bands of the spectrum all the way from x-rays through the ultraviolet and visible into the infrared, the physicist measuring by various means the energy absorbed or transmitted in the wave-lengths of interest for the specific purpose while the chemist selects batch mixtures which give not only appropriate filters for the radiated energy but also glasses normal in other respects.

Such, for instance, are the glasses used by physicians and dentists and others for protection against x-rays and on the other hand glasses for effective transmission of x-rays. Other glasses either transmit ultraviolet, some as well as pure silica glass, or, like the glass used in so-called sunlight lamps, transmit in the ultraviolet the therapeutic wave-lengths only. A certain glass transmits ultraviolet but absorbs all visible light, and is used with fluorescent materials to produce weird stage effects.

One glass, clear and almost colorless, transmits practically all the visible light but absorbs practically all invisible heat rays. Another glass lets through no visible light but is fairly transparent to infrared. For air-conditioning there are glasses which while cutting out the glare of the sun, as well as most of its heat, have low expansion to guard against their breaking as they are heated by the absorbed energy. There are glasses which with the incandescent lamp will give you for color comparison whichever of the several varieties of sunlight you wish: direct sun, northern sky, or overcast sky. In all such the handiwork of the physicist is evident.

The widely used opal translucent glass shades and bowls and globes for electric lamps, as well as the frosted bulb which now has so largely replaced the clear bulb, are based upon the physicist's measurements of diffusion and transmission.

The automobile front glass which deflects to the road and makes useful the otherwise glaring beam from the headlights we owe to the physicist.



Glass construction units, hollow, and internally ribbed to provide image obscuration, replace bricks in the building of modern walls.

The classic collaborations between chemist and physicist in the glass field should be mentioned here even though the physicists concerned were in university positions rather than in industry. Harcourt, the chemist, with Stokes, the physicist, and later Schott, the chemist, with Abbe, the physicist, laid in the nineteenth century the foundations of scientific glass composition development from which sprang directly the modern microscope and other optical instruments which made possible, among other scientific advancement, present-day biology with its vast contribution to the well-being of mankind.

In the recent seemingly magical art of drawing fibers and weaving textiles of glass the physicist makes constant checks of the heat-insulating value of the material. His measurements of electric resistance give him reason to think the fiber woven into the proper fabric has merit as an insulating cover for electric wire and cable. He has already demonstrated that these fibers, a few ten thousandths inch in diameter, have a tensile strength sometimes as great as a million pounds per square inch, greater than that of steel. He knows, therefore, that glass intrinsically

is a fairly robust substance and he may be pardoned for dreaming of the day when he can incorporate its full strength into ordinary glassware and so approach the perfect form of matter.

Under the sponsorship of the physicist heat treatment of glass is assuming increased practical importance. Annealing, required to prevent stresses which might cause spontaneous breakage, was a haphazard process, wasting time and fuel, and causing loss of ware either by breakage because of too little heat or by melting down because of too much heat, until the physicists worked out formulas for temperatures and rate of cooling.

As with steel, so with glass, heat treatment affects the physical properties even when strain is absent. Actually the extremely viscous nature of glass tends to slow up or stop altogether the internal rearrangements which normally are caused by change in temperature. As you cool water, say from boiling to room temperature, you increase the density. The change in density takes place with the change in temperature. Glass, however, is such a sluggish liquid—if it is a liquid—that the changes in density and other properties require time, and if it is cooled fast to ordinary temperatures, at which it ceases to flow, the density and other properties are caught in the process of changing and the high temperature values are so to speak frozen into the cold glass. It is as if you could cool molasses to its proverbial January temperature and still keep it as freely flowing as say in November. Very slow cooling, on the other hand, enables the internal rearrangement to keep pace with the temperature change and thus produces the normal properties in the cooled glass. It is, therefore, possible to have two glasses of identical chemical composition, one cooled rapidly and one slowly, both free from elastic strain and yet differing by several tenths of a percent in physical properties. This discovery throws light on difficulties encountered in accurately reproducing a glass and points out the remedy. To control properly the refractive index of optical glass, for instance, the heat treatment must be precisely specified.

Physics has been applied in heat treatment of another kind to increase four- or fivefold or more the strength of glass. In the city of Paris today your taxicab is equipped with heat-treated safety glass windows, not double sheets with plastic layers between, as in this country, but a single sheet which will withstand four or five times as hard a blow as ordinary glass of the same thickness and which, when it does break, goes into harmless fragments the size of the end of your finger rather than into sharp, jagged splinters.

To get this result the sheets have been heated and then suspended before a multitude of fine air jets. The surface layer of the glass is thus thrown under compression and the interior under tension. The tensile strength of the sheet is increased by the amount of the surface compression—for a force tending to stretch the glass must first overcome such compression—and it is tensile strength which ordinarily determines whether or not glass will break.

Glass frying pans in which ham and eggs, griddle cakes, fried potatoes, or any of the ordinary products of the oldtime skillet, can be prepared are a recent development in which the physicists have played an indispensable role. Glass saucepans, double boilers, as well as skillets of thick glass can be used directly over the gas flame or electric heater. In combining the necessary physical properties the physicists have solved a most delicate and difficult problem.

The production of the disk for the mirror of the large California Institute of Technology telescope, an achievement in glass-making which recently attracted public attention, was almost entirely the work of the physicist once the chemist had provided the proper glass formula. It was a task which involved pioneering at every stage. An object of unprecedented size in glass, a disk some seventeen feet across and twenty-seven inches thick, weighing twenty tons, flat on one side and with ribs on the other, was to be formed of a lower expansion borosilicate glass than any in commercial use, and was to be annealed as perfectly as the objective in a microscope. There was no established way of melting such a glass for such a purpose, nor, once it was melted, of giving it the form of a ribbed disk. At every step until the glass was safe in California new methods had to be worked out and special equipment devised.

Where failure in any one of dozens of details of planning or execution might have wrecked the enterprise at the cost of a small fortune finan-

cially and a heartbreaking delay, the project was carried through successfully by the skill and ingenuity and courage of the industrial physicist. By no means all of the difficulties encountered were problems in physics but they were all incidental to an undertaking for which no one could be better prepared than the physicist, and they illustrate well the kind of accomplishment for which the right physicist may safely be called upon.

Aside from these very practical matters with which the physicists are engaged they are at work also on purely scientific problems in connection with glass, the solution of which, we are convinced, in the end will be no less fruitful than effort applied directly today to manufacturing methods or to products.

By reciting this catalog of some of his activities and accomplishments I have tried to indicate how heavily we lean on the physicst for consultation, for setting and maintaining standards of quality, for overcoming manufacturing difficulties and for seeking out new fields for glass. If our experience is a fair criterion, the trained physicist will in future be more and more in demand as industry learns his worth and comes to realize that to his hands can best be entrusted some of its most vital problems.

Individual initiative! It is the backbone of scientific progress. Perhaps the adjective "scientific" is quite superfluous. But we are talking about physics. How many individuals have contributed to the development of this relatively small area in physics which I have outlined to you in this paper? And in what countries have they worked? England, France, Sweden, Germany, Holland, Italy, America. Though associated together in no formal organization, yet, because of their common interests, they are engaged in what amounts to a most effective cooperative enterprise, if one is to judge by results. They form a sort of international league for the promotion of science, from which strife, dissensions, recriminations are all but absent. Through the comparatively unorganized, but most effective, exchange of scientific literature each member of the league knows what the others are doing. When one member announces a new theory or a new discovery, the others either accept it at face value, or at most try to find out whether he is right. Seldom do they deliberately set out to prove him wrong—a very different, not to say destructive, attitude. If physics has made such astounding advances during the past thirty-five years, it is largely because of this fine spirit of international tolerance, good-will and respect among physicists. No individual, no laboratory, no country has had a monopoly on the making of contributions. And what applies to physics applies to all the natural sciences.

—F. K. RICHTMYER, Presidential Address before the American Physical Society, December 29, 1936.

Contributed Original Research

The Effect of Pressure on the Modulus of Rigidity of Several Metals and Glasses

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The effect of pressure on the modulus of rigidity has been determined for five metals and two glasses, to 4000 kg/cm², by a dynamical method. The corrections due to the viscous motion of the pressure medium are computed approximately and shown to be negligible. Absolute values of the dynamical Young's modulus and modulus of rigidity are also given, and the derived compressibilities compared with directly measured compressibilities.

INTRODUCTION

The effect of hydrostatic pressure upon the modulus of rigidity of a number of metals and glasses has been studied by Bridgman[1] using a statical method. The materials were made into helical springs and the elongation of these springs under known forces was determined at different pressures. This method is inapplicable to materials which flow plastically at low stresses, or which cannot be formed into springs. In the present work, a dynamical method has been utilized, which permits the study of soft metals as well as of hard, and which may eventually prove suitable for the determination of the rigidity of such materials as rocks under pressure. This method has already been applied by Ide[2] to the measurement of the rigidity of rocks at ordinary pressure.

It is well known that bars or rods possess characteristic frequencies of free vibration in various modes; in the case of torsional vibration of a right circular cylinder of uniform section, isotropic and elastic and free from surface tractions, exact solutions of the equations of elastic theory may be obtained. The fundamental frequency depends only upon the length, the density and the modulus of rigidity. While the postulated conditions are never exactly realized experimentally, they may often be approached so closely that the required corrections are small and may be calculated with ample precision. On account of the rapid decay of the free vibrations produced by internal friction, it is more convenient to work with forced oscillations of small amplitude. The frequency of forced oscillations for which the energy is a maximum, which is the resonant frequency observed in this work, is practically independent of the internal damping.

For the present purpose, the rod must be made to vibrate in contact with some fluid which is required to exert the pressure. Since in torsional vibration there is no change of volume of the vibrating body, the motion of its surface being purely tangential, the pressure medium affects the motion only through surface tractions due to its viscosity, aside from the changes of dimensions and rigidity due to the hydrostatic pressure. A rigorous study of the motion of a viscous fluid in contact with a cylinder set into torsional vibration does not appear to have been made, although it does not seem unduly difficult. The order of magnitude of the surface tractions may, however, be computed by using a solution given by Stokes for the traction on a plane in tangential, oscillatory motion in a viscous medium; for the wave-length of the shear wave in the medium is small with respect to the radius of the rod. The effect of these tractions, and of the various perturbing elements upon the resonant frequency may then be found by a method of approximation frequently used by Lord Rayleigh in his *Theory of Sound*.

APPARATUS AND PROCEDURE

The general scheme of mounting and driving the specimen is indicated in Fig. 1. The specimen (*A*) is a right circular cylinder about 20 cm long and 1.6 cm in diameter (in several cases the diameter was as little as 1.0 cm). It is mounted vertically in a brass tube (*H*) on steel pins, 0.107 cm in diameter, coaxial with the specimen. The bottom pin is clamped at *E*, at a distance of two or three centimeters from the end of the specimen, preventing rotation or sliding of the rod as a whole; the upper pin slips freely into a centering bearing.

The pole pieces (*F*) pass through the coils (*B* and *C*) and screw into the ends of permanent horseshoe magnets which are not shown in the figure but are held in two massive brass cylinders, one of which is marked *G*. These magnets polarize the soft transformer iron flanges (*D*) fixed to the ends of the rod; when an alternating voltage is applied to the coils at one end, an alternating torque of the same frequency is thus applied to this end of the rod. The motion of the other end is indicated by the voltage induced in the other set of coils, which are connected to a powerful amplifier. The frequency of the impressed voltage is varied until a resonant frequency of the rod, as shown by the amplifier output, is found.

This disposition is identical in principle with the arrange-

[1] P. W. Bridgman, Proc. Am. Acad. Arts and Sci. **63**, 401 (1929), and **64**, 39 (1929).
[2] J. M. Ide, Proc. Nat. Acad. Sci. **22**, 482 (1936).

ments used by Wegel and Walther[3] and by Ide,[2] with the difference in detail, important for the present work, that as shown in Fig. 1, the flanges (D) are here turned so as to present only a thin edge, instead of a wide surface to the direction of motion. These flanges were 3 mm wide, 0.38 mm thick; the vertical projecting parts were 3 mm long. The air gap between these pieces and the pole pieces was usually about 0.8 mm. The action of the medium upon these flanges was consequently almost entirely of viscous origin; the thin edges alone sent out compressional waves. The effect upon the resonant frequency of compressional waves from large flanges, though perhaps negligible in air at one atmosphere, may become very appreciable at high pressures.

The driving voltage was supplied from a General Radio Company Type 377B low frequency oscillator, with a power amplifier. The frequency of this oscillator is extremely stable with regard to variations of battery voltages, output, temperature; if the indicated adjustments are made, the frequency at a given setting will remain constant over long periods of time to about one part in ten thousand. The oscillator was calibrated by beating its output against the harmonics of the crystal controlled standard frequency generator of the Cruft Laboratory. For interpolation, a formula of the type $f^2=K/(C+C_0)$ was found to fit the observations fairly closely; here f is the frequency, C the setting of the oscillator condenser, C_0 and K constants. The average deviation over the whole scale of the observed from the computed frequencies was of the order of one-half percent; this is several times larger than the probable error of the calibration. The absolute value of the frequency is however of secondary interest for the present problem; the change of frequency with the pressures employed could usually be obtained by variation of the small air condenser of the oscillator, and over this small range an expression of the form given above probably holds with a good approximation, giving for the relative change of frequency $df/f=-(dC/2)/(C+C_0)$.

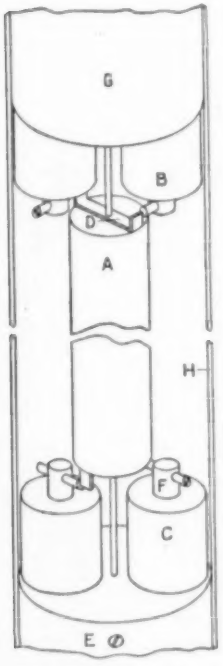

FIG. 1. Disposition of specimen and driving units.

A four-stage, resistance-coupled amplifier connected to the receiving coils increased the voltage induced in these coils until it could be read conveniently on a small voltmeter. Since for ease of operation, it was placed only a few feet from the oscillator, the amplifier and its leads required careful shielding. It was possible in most cases to reduce the background to less than one-fifth of the indication at resonance, and most of the residual background came from direct magnetic interaction between the driving and receiving coils.

The brass tube (H, Fig. 1) in which the specimen and the exciting and receiving units were mounted, was fixed to a steel plug of the mushroom type devised by P. W. Bridgman. Two insulated, pressure-tight steel stems passed through the plug and were used for electrical connections to the oscillator and the amplifier. Inside the tube, the amplifier lead was shielded by a heavy steel tube enclosing the wire and reaching from the steel plug nearly to the receiving coils at the bottom. The brass tube and plug fitted easily into a heavy-walled chrome-molybdenum steel cylinder, from which a pressure connection led to a hydraulic press. Nitrogen was run into the apparatus at about 100 atmospheres from commercial tanks. A cylinder of large internal diameter with a moving piston was used in conjunction with a motor-driven pump to raise the pressure of the nitrogen to 1000 atmospheres; the hydraulic press was then capable of increasing the pressure to 4000 atmospheres. A complete description of this type of apparatus may be found in a paper by Birch and Law.[4] The pressure was measured with an uncertainty of about one-half percent by the change of resistance of a calibrated 200-ohm manganin gauge coil, connected to a Carey Foster bridge.

The cylinder containing the specimen was water-jacketed; a stream of water was pumped through the jacket from a tank in which the water was thermostatically maintained at a constant temperature, about 30°C in most cases. Below 500 atmospheres, a considerable time was required to bring the specimen to the equilibrium temperature; the only metallic contact was through the small steel pins at the ends, and the nitrogen was still a poor agent for the transfer of heat. At higher pressures, this time was much reduced.

Observations were usually made at intervals of one thousand atmospheres on increasing the pressure, and halfway between on decreasing the pressure. Temperature-pressure equilibrium having been attained, an observation consisted in studying the amplifier output as function of the oscillator condenser setting. In general, the fundamental torsional frequency was easily identified; the type of drive and pick-up were such as to respond strongly only to the torsional modes, although flexural modes could sometimes be found. Higher harmonics of the torsional mode could also be observed, but usually only the fundamental frequency was followed. The resonance curves of the metals were extremely sharp, a change of the oscillator condenser setting of one division in the last place being sufficient in the case of steel or nickel to pass from one side of the peak to the other. No change of frequency with driving voltage was observable for the range of driving voltage available. The strains involved in the vibration must be very minute; although no effort was made to determine their magnitude, it seems certain that they did not exceed 10^{-5}, and probably they were much smaller.

[3] R. L. Wegel and H. Walther, Physics **6**, 141 (1935).

[4] F. Birch and R. R. Law, Geol. Soc. Am., Bull. **46**, 1219 (1935).

Theory

In the absence of an exact solution of the problem of torsional vibrations of a cylinder in a viscous medium, we may adopt Rayleigh's method[5] of approximation to find the small effect of the various perturbing factors upon the resonant frequency. Using the solution for the free vibration with no surface tractions and no loading, we compute the corrections to the energy terms due to the various constraints, tractions and so on, assuming the form of the vibration to remain unchanged. The boundary conditions will no longer be satisfied exactly, but if the perturbing factors are small, we may expect to get the right order of magnitude for the change in the characteristic frequency. Furthermore, since we are here primarily interested in the effect of pressure, the greatest importance attaches only to those corrections which change with pressure. It turns out that not only are the corrections themselves small but the effect of pressure upon the corrections is small, so that the various approximations involved appear to be fully justified.

For frequencies below a certain critical frequency (which is very high for all of the present specimens), the equations of small motions, in the case of zero surface tractions and no internal dissipation, admit solutions[6] of the form $u_r=u_z=0$, $u_\theta=B_n r\cos(n\pi z/L)\cos[(n\pi t/L)(\mu/\rho)^{\frac{1}{2}}]$, where u_r, u_θ, and u_z are the displacements in terms of the polar coordinates r, θ and z, with the origin at the center of one end of the rod. The length of the rod is L, the density ρ, the modulus of rigidity μ, and n is any integer. It will be sufficient to consider the lowest frequency alone, where $n=1$. If the rod is subjected to the hydrostatic pressure p, exerted by a frictionless fluid, we may still obtain an exact solution. In this case, the equations of motion and the boundary conditions are satisfied if u_r and u_z are equal to Cr and Cz, respectively, with $C=p\beta/3$, where β is the cubic compressibility, and if u_θ is given by the same expression as before, with the L, ρ and μ corresponding to the pressure p. The change of the fundamental frequency may be found from the relation, $2fL=(\mu/\rho)^{\frac{1}{2}}$; differentiating logarithmically, we have $2df/f=d\mu/\mu-2dL/L-d\rho/\rho$, and since $d\rho/\rho=-3dL/L$, $2df/f=d\mu/\mu+dL/L$. This is in fact the relation which we find we can use to determine the change of the modulus of rigidity with pressure.

We now suppose that in the more general case of forced vibration with small dissipative forces and mechanical constraints, the above solution for the displacement u_θ will still be approximately valid for frequencies in the neighborhood of resonance. A single coordinate will then suffice to describe the motion; it is convenient to take the angular displacement at one end of the rod, say u_θ/r for $z=0$, which will be called ϕ. Then the kinetic energy will be $T+\Delta T=\frac{1}{2}(a+\Delta a)\dot\phi^2$, the potential energy, $U+\Delta U=\frac{1}{2}(c+\Delta c)\phi^2$ and the frequency of resonance f will be given by $\omega^2=(c+\Delta c)/(a+\Delta a)$, where $\omega=2\pi f$, or $\omega^2/\omega_0^2=(1+\Delta c/c)/(1+\Delta a/a)$, with $\omega_0=(c/a)^{\frac{1}{2}}$, the resonant frequency of the unperturbed rod.

It is readily found that $a=(\pi/4)\rho R^4L$ and $c=\pi^3\mu R^4/4L$, R being the radius of the rod. The term Δc is due to the twist of the clamping pin, and equals $\pi\mu'(r')^4/2L'$ where r' is the radius of the pin, L' the length to the clamping point, and μ' the rigidity of the pin. Each of the flanges contributes to Δa a part equal to I, the moment of inertia of the flange about the axis.

To calculate the effect of the viscous forces at the surface we may use a solution given by Stokes[7] for the tangential oscillation of a plane, since the wave-length of the highly damped shear wave sent in to the fluid is small in comparison with the radius of the rod for all of the frequencies used in this work. This wave-length is given by $(4\pi\nu/f\delta)^{\frac{1}{2}}$; at about 5000 cycles per second, the wave-length in air at one atmosphere is 0.02 cm, in nitrogen at 1000 atmospheres, 0.0014 cm. For the plane, the traction per unit area of the surface is $-(\pi f\delta\nu)^{\frac{1}{2}}(v+\dot v/2\pi f)$ where v is the tangential velocity, ν the viscosity and δ the density of the fluid. Multiplying this by v to get the rate at which energy is communicated to the fluid, and integrating over the surface of the rod, we find $\pi R^3(R+L)(\pi f\delta\nu)^{\frac{1}{2}}(\dot\phi^2+\phi\ddot\phi/2\pi f)$. Besides the dissipation represented by the term in $\dot\phi^2$, there is an addition to the kinetic energy of the system which is a function of the frequency. Using the measurements of Michels and Gibson[8] of the viscosity of nitrogen, we may evaluate the contribution to Δa at the resonant frequency. At 985 atmospheres and 25°C, $\nu=520\cdot10^{-6}$, $\delta=0.568$. Calculations for a number of the specimens show that this amounts to less than one part in ten thousand of the whole kinetic energy. The viscosity of nitrogen has not been measured at higher pressures, but a rough extrapolation of the data of Michels and Gibson indicates that the function $(\nu\delta)^{\frac{1}{2}}$ is at 4000 atmospheres not much if any greater than twice its value at 1000. Thus the error in frequency involved in neglecting the effect of the medium is at most of the order of one part in ten thousand, which is within the precision of the measurements.

The fraction of the potential energy of the system due to the twist of the clamping pin is $4\cdot10^{-5}$ in the case of the steel specimen and several times as great for some of the specimens of low rigidity. The fraction of the total kinetic energy due to the flanges is more important; this is $3.8\cdot10^{-3}$ for the steel and larger for the lighter materials. The change with pressure of this correction is due to the difference of compressibility of the flanges and the specimen. It is zero for the steel; for Pyrex glass, the most compressible material studied, it amounts to about $6\cdot10^{-3}$ for 4000 atmospheres, and since this is applied to an energy correction of about 4 percent, its effect on the frequency is of the order of 10^{-4}.

Although no account has been taken of the effect of internal damping upon the frequency, nor of the effect of pressure upon the internal damping, it is clear that both of these are small. Resonance is extremely sharp for the metals, and in spite of the additional damping due to the medium, the resonance curve shows little broadening at high pressure. In the case of the glasses, the losses were chiefly due to the various waxes and cements, which were used to fasten the driving flanges, and might increase or decrease with pressure, depending upon the properties of

[5] Lord Rayleigh, *The Theory of Sound*, Vol. 1, p. 111 *et seq.*
[6] A. E. H. Love, *A Treatise on the Mathematical Theory of Elasticity*, fourth edition (Cambridge University Press, 1927), p. 287.
[7] Sir George G. Stokes, Math. and Phys. Papers, Vol. III, p. 20.
[8] A. Michels and R. O. Gibson, Proc. Roy. Soc. **A134**, 8 (1931).

these materials. The losses properly attributable to the glasses themselves could not be separated out at high pressure, but measurements made at one atmosphere using hard De Khotinsky wax for fastening the flanges showed that the internal dissipation was small and of the same order as that found by Wegel and Walther. The effect of the internal damping upon the frequency of resonance is considerably less than one part in ten thousand for all of the present materials.

There remains a correction which it is difficult to calculate but which is conceivably more important than the others. This is due to the compressional waves generated by the thin edges of the flanges. An empirical study of the effect may be made by using flanges of different thicknesses for pressure runs with a given specimen. This was done with the steel specimen, with a flange area normal to the direction of motion about ten times as great as the usual area. The effect on the apparent pressure change of the steel was very slightly greater than the probable error of setting and in the direction to be expected. It seems legitimate to conclude that the error of neglecting this factor when the thin flanges are used is not greater than the other uncertainties.

Finally, we may conclude that the resonant frequency is given with this experimental arrangement, by the relation, $2fL=[\mu(1+\alpha)/\rho]^{\frac{1}{2}}$ where α is a correction function sensibly independent of the pressure. The relation between the pressure coefficient of rigidity and frequency at constant temperature is consequently,

$$\frac{1}{\mu}\left(\frac{\partial\mu}{\partial p}\right)_T=\frac{2}{f}\left(\frac{\partial f}{\partial p}\right)_T-\frac{1}{L}\left(\frac{\partial L}{\partial p}\right)_T.$$

Results

In the case of the metals the runs were very regular, deviations from linearity being for the most part of the same order as the precision of the condenser setting. It was generally found that temperature equilibrium could not be obtained at atmospheric pressure within any reasonable time, so that the initial point usually failed to lie on the straight line determined by the points at higher pressures. The effect of a change of temperature of about 1°C is equivalent to that of a change of pressure of 200 kg/cm² for the nickel, with roughly comparable figures for the other metals. The random departures from linearity observed for the metals are almost certainly due to small fluctuations of temperature.

The runs with silica glass and Pyrex glass were much less regular; although for these glasses the influence of small temperature changes is insignificant, the problem of fixing the armatures in such a way that their coupling to the rod remained invariable as the pressure was changed was never perfectly solved. Various waxes, Canada balsam, glyptal cement and so on, were tried, with varying success. In most cases, there was a tendency for the nitrogen to emulsify the plastic material, thus changing the energy correction for the flanges as the pressure and the state of the emulsion changed. A very hard wax gave good results at low pressures, but cracked at higher pressures. Aside from the specific properties of the cement, the coupling of the armature to glass is a difficult problem on account of the very different compressibilities of glass and iron. It was possible, however, by combining several runs, to obtain reasonably consistent results. The changes of modulus are large for both of these glasses, and the final figures for the pressure coefficient have nearly the same relative precision as those for the metals. For the silica glass, three runs were plotted on the same scale and a straight line passed through the whole set of points. This represents the results with all the precision justified by the observations, although there is a noticeable tendency for the rate of decrease of the rigidity to become greater as the pressure increases. In the case of the Pyrex, four runs were used separately to give four coefficients, of which the weighted mean only is given. The average relative difference from the value adopted was about three percent.

Table I. *Pressure coefficient of rigidity at 30°C.*

Material	Density	Bridgman $\frac{1}{\mu}\left(\frac{\partial\mu}{\partial\rho}\right)_T\cdot10^6$	This paper $\frac{1}{\mu}\left(\frac{\partial\mu}{\partial\rho}\right)_T\cdot10^6$	This paper Average deviation %
Drill rod	7.838	2.16 (piano wire)	2.36	2
Nickel	8.842	1.8	1.82	2.5
Copper (commercial electrolytic)	8.904		2.71	1
Aluminum (commercial)	2.717		7.61	0.5
Monel	8.826		1.77	2
Pyrex glass	2.225	−8.45	−8.3	2.7
Silica glass	2.208		−10.1	3.1

This effect has not hitherto been studied for copper, monel, aluminum or silica glass. In the case of steel, nickel, and Pyrex, comparison is possible with the results of Bridgman[1] which are shown in Table I, along with the values of this paper. All are for 30°C. The unit of pressure is the kilogram per square centimeter. The agreement is evidently well within the experimental uncertainty.

The decrease of rigidity of silica glass with pressure is larger than that found for any other glass, and there appears to be a connection between this effect and the anomalous increase of compressibility with pressure, also exhibited by Pyrex. There are, however, some glasses, studied by Bridgman, which show the one kind of behavior but not the other. The measured rate of decrease of rigidity for silica glass is very close to that assumed by Birch and Dow,[9] which, combined with the observed change of compressibility with pressure, gave an estimated decrease of Young's modulus of 12 percent for 10,000 kg/cm².

It was possible, without much additional work, to obtain the dynamical values of Young's modulus for these specimens at one atmosphere by the method of electrically excited longitudinal vibrations described by Ide;[10] these are listed, together with the values for the rigidity, in Table II, all for one atmosphere and about 30°C. The absolute values of the frequencies were determined with an uncertainty of about one-half percent, giving a precision

[9] F. Birch and R. B. Dow, Geol. Soc. Am., Bull. **47**, 1235 (1936).
[10] J. M. Ide, Rev. Sci. Inst. **6**, 296 (1935).

TABLE II. *Observed values of the dynamical moduli; derived and measured compressibilities.*

	$E \cdot 10^{-11}$ dynes/cm²	$\mu \cdot 10^{-11}$ dynes/cm²	σ calculated	$\beta_S \cdot 10^{13}$ cm²/dyne calculated	$\beta_T \cdot 10^{13}$ cm²/dyne calculated	$\beta_T \cdot 10^{13}$ cm²/dyne observed
Steel	20.90	8.11	0.288	6.07	6.16	5.99
Nickel	21.43	8.02	0.336	4.58	4.67	5.39
Copper	12.77	4.68	0.364	6.4	6.6	7.34
Aluminum	7.10	2.62	0.355	12.7	13.4	13.7
Monel	18.39	6.72	0.368	4.3	4.4	(5.4)*
Pyrex	6.21	2.52	0.232	25.9	25.9	30.7
Silica glass	7.21	3.05	0.181	26.5	26.5	26.9

* Taken equal to the compressibility of nickel.

of one percent for the moduli. From these two moduli were calculated Poisson's ratio σ, and the cubic compressibility β, using the connecting equations for isotropic elastic bodies, $\sigma=(E/2\mu-1)$, $\beta=9/E-3/\mu$. Since for the very small strains involved in these measurements, no serious departure from elasticity can be expected, the differences between the compressibilities calculated in this way, and the measured compressibilities are to be ascribed chiefly to lack of isotropy.

A difference is, however, due to the fact that the compressibilities calculated from the dynamical moduli correspond very nearly to the *adiabatic* values of compressibility, whereas the measured compressibilities, taken from the results of Bridgman, are obtained by a static, isothermal method. The difference between the isothermal (β_T) and adiabatic (β_S) compressibilities is surprisingly large for some of the metals. The relation is

$$\beta_T-\beta_S=(T/C_pV)(\partial V/\partial T)^2{}_p$$

where C_p is the specific heat at constant pressure. Consequently β_T is always greater than β_S. The difference amounts to 5.2 percent at 30° for aluminum; it is negligible for the glasses at ordinary temperatures. The isothermal compressibility β_T as deduced from these dynamic measurements is given in Table II for comparison with the directly measured β_T. It is also to be remembered that the uncertainty in E and μ produces a possible error roughly three or four times as great in the derived compressibility.

The discrepancies between the computed and observed values of β_T are greater than 4 percent for the nickel, copper, monel and Pyrex, and indicate anisotropy in these cases. The agreement for the steel, aluminum and silica glass may be considered good. It is worth while remarking that this set of values for silica glass are more nearly mutually compatible than any given heretofore. The present value for the rigidity agrees well with that observed by Horton[11] which is about $3.02 \cdot 10^{11}$ at 30°. The figures to be found in the literature for E are all lower than, and some scarcely more than half of the value obtained here. The nearest set are those of Stumpf,[12] who gives $E=6.99 \cdot 10^{11}$ and $\mu=3.08 \cdot 10^{11}$; Heymans and Allis[13] find $E=6.89 \cdot 10^{11}$ and $\sigma=0.138$, whence is derived $\mu=3.03 \cdot 10^{11}$. Both of these sets lead to values of the compressibility about 14 percent too high, as against 1.6 percent too low for the present set.

It is a pleasure to acknowledge the writer's indebtedness to Dr. John M. Ide for an introduction to the possibilities of the dynamical methods of studying the elastic constants, and for very valuable help in the design and choice of equipment; to Mr. Dennison Bancroft for many of the measurements, for the oscillator calibration, and for checking the computation of corrections; and to the Committee on Geophysical Research of Harvard University for the interest and support which has made this work possible.

[11] F. Horton, Phil. Trans. Roy. Soc. Lond. **A204**, 407 (1905).
[12] F. Stumpf, Ann. d. Physik **40**, 879 (1913).
[13] P. Heymans and W. P. Allis, J. Math. Phys. Mass. Inst. Tech. **2**, 216 (1923), or see R. B. Sosman, *The Properties of Silica* (1927) for a critical discussion of the available data.

The Electrical Conductivity of Nickel-Sulphur Electrodeposits

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Under certain conditions, an electrodeposit from an aqueous electrolyte containing essentially nickel ammonium sulphate and sodium thiosulphate has an amorphous crystal structure and contains approximately 13 percent S and 87 percent Ni. It is shown that if this deposit is warmed to about 170°C its electrical resistance suddenly decreases and its structure changes to crystalline, without appreciable change in weight.

X-RAY diffraction photographs show that some elements and combinations of elements are electrodeposited in an amorphous state which changes to crystalline when the deposit is warmed.[1,2] The authors have recently found that nickel and sulphur electrodeposited simultaneously under certain conditions have the properties[3] mentioned and that the transition from amorphous to crystalline takes place at a temperature much lower than the melting point of the electrodeposit, without appreciable loss of weight. One is led to suppose that the heating

[1] H. Kersten, Physics **2**, 276 (1932).
[2] H. Kersten and Joseph Maas, Physics **3**, 137 (1932).

[3] A study of the simultaneous electrodeposition of nickel and sulphur will appear as Preprint No. 71-4 of The Electrochemical Society on May 3, 1937.

merely makes it possible for the atoms to move from their more or less disordered positions as electrodeposited, to a more orderly arrangement in a space lattice. Since the nickel-sulphur electrodeposits are reasonably good conductors it seemed of interest to measure the electrical resistance of a sample during the transition period.

Method and Results

The plating bath used had the following composition: nickel ammonium sulphate ($NiSO_4(NH_4)_2SO_4 : 6H_2O$), 100 g; sodium thiosulphate (hypo) ($Na_2S_2O_3 : 7H_2O$), 10 g; sodium citrate ($Na_3C_6H_5O_7 : 5H_2O$), 15 g; water, 1 liter. The electrodeposition was carried on in a one-liter beaker, using a stainless steel cathode 5 cm wide immersed to a depth of 9 cm, placed 4 cm from a lead anode having the same dimensions. The pH of the bath at the beginning of electrolysis was 6.4, but, on account of the insoluble anodes this value decreased as the electrodeposition progressed. The temperature of the bath was held at 30°C and the current density at 0.55 amp./dm^2. Under these conditions the deposit contained approximately 13 percent S and 87 percent Ni, and reached a thickness of about 0.06 mm in 2 hours.

Since the deposit did not adhere well to the stainless steel it was possible, by bending the cathode, to cause the deposit to break off in strips about 1 mm wide. The samples used for the conductivity determinations then had the dimensions of about 1 cm by 1 mm by 0.06 mm. The ends of one such strip were clamped between small brass plates which were immersed together with the sample in oil and connected in an electrical circuit so that with a current of 0.25 amp. passing through the sample the voltage drop across it could be measured with a potentiometer while the temperature of the oil and the immersed sample was gradually raised at a rate of about 3°C per minute.

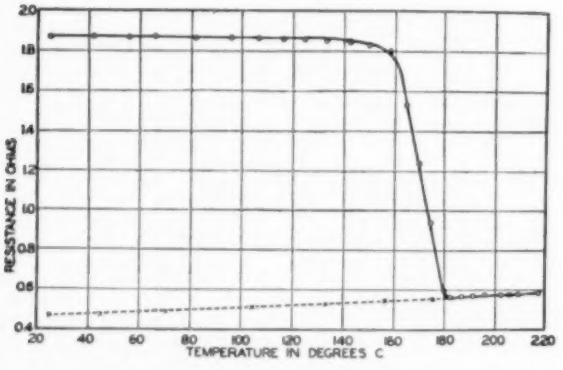

Fig. 1. Showing how the resistance of a typical sample of a nickel-sulphur electrodeposit changed with temperature.

Fig. 1 shows how the resistance of a typical sample changed as the temperature was raised from 20°C to 220°C. After a sample had been heated past 180°C its resistance changed with temperature according to the lower curve of Fig. 1 for both increasing and decreasing temperatures. On account of the brittleness of the sample it was impossible to prepare one of such uniform cross section that its specific resistance could be determined.

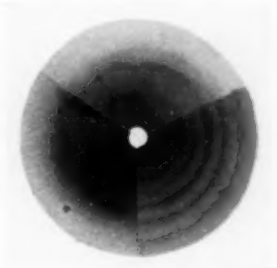

Fig. 2. Showing how the structure of a typical sample of a nickel-sulphur electrodeposit changed from amorphous to crystalline while being heated. The temperatures to which the samples were heated are: For the left sector, 160°C; top sector, 170°C; right sector, 180°C.

Samples taken from the same piece as that from which the one used for the resistance determination was taken, were placed in oil with the one whose resistance was being measured and removed at intervals as the temperature increased. Laue pictures of these were made and it was found that from room temperature to about 160°C the structure was amorphous, while between 160°C and 180°C the sharp rings began to appear in the Laue pictures indicating that the samples were changing from amorphous to crystalline. Above 180°C the structure was like that reported for Ni_3S_2 by Alsen,[4] which is simple cubic with $a_0 = 4.08$A. Representative Laue pictures taken with the radiation from a copper target with a sample-to-film distance of 2 cm are shown in Fig. 2. Since the nickel in the sample acted as a filter for the beta line, no rings from that line appear in the photographs.

In order to determine whether the sample lost any weight during the transition from amorphous to crystalline, a sample was dried at about 110°C for 2 hours. This temperature was not high enough to cause the structure to change from amorphous to crystalline. The sample was then weighed, heated to 200°C and again weighed. After this heating it had changed from the amorphous to the crystalline state. The weight before the final heating was 0.2478 g and that afterward 0.2472 g.

[4] Nils Alsen, Geologiska Foreningens i Stockholm Forhandlingar **46**, 52 (1925).

A Method for Measuring the Rheological Properties of Materials of Great Consistency Such as Asphalts

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It has been found that when an ASTM penetrometer needle is allowed to sink into a material such as asphalt for a series of successive time intervals, the rate of flow varies linearly with the shearing stress as calculated from these equations:

$$a=\frac{7.854\times10^{-5}(P_f-P_i)}{t}, \qquad b=\frac{0.098\times10^4\,W}{0.001571(P_f+P_i)-0.0521},$$

when a = rate of flow in cm³/sec.,
b = shearing stress in dynes/cm²,
P_f = final penetration during interval t, in decimillimeters,
P_i = initial penetration during interval t, in decimms.,
t = time interval in seconds,
W = weight of needle and instrument head in grams.

The first penetration in hard materials and the first several penetrations in soft materials give erratic data; otherwise no systematic errors have been observed, and accuracy depends upon the skill of the operator. As expected from plastometric theory and from previous work by other investigators: (1) Results are almost independent of the shearing stress used; (2) The mobility is affected by the shape of the surface of the material at the point of punch, while the yield value is unaffected; (3) Minus log m, which is mathematically equal to log viscosity when $f=0$, varies linearly with the temperature. (4) Yield values for asphalts from a particular crude stock vary linearly with the percent asphaltene content—the concentration of zero yield value being at 21.8 percent. (5) On aging, an asphalt increases in yield value and decreases in mobility. The author finds that on air-blowing two particular charge stocks, the melting point can be predicted from the mobility at 77 degrees F:

$$\text{m.p.}=(-\log m-r)/s+77,$$

when r and s are constants characteristic of the charge stock.

FOR any plastic solid, $m=a/(b-f)$ when m = mobility in reciprocal poises, a = rate of flow in cm³ per sec., b = shearing stress in dynes per cm², and f = yield value in dynes per cm². When a for different shearing stresses is plotted against the corresponding value of b, a straight line is obtained whose slope is the mobility and whose shearing stress intercept is the yield value of the plastic solid.[1] If $f=0$, the material is a liquid whose viscosity $=b/a$ poises.

These relationships are illustrated in Fig. 1 which represents the flow properties of various asphalts as determined with a New York penetrometer in the following manner:

The sample is prepared, brought to temperature, and placed under the needle as prescribed by the ASTM.[2] The needle is released for a measured number of seconds and the resulting penetration is recorded (in decimillimeters). Without touching the needle or the sample, the needle is now released for a series of successive time intervals and the resulting penetration noted after each interval. As the needle sinks deeper into the sample, the time intervals will have to be increased in order to get a measurable increment of penetration for the interval. The volume of flow of the material during any time interval is taken to be equal to the additional volume of the needle submerged during that interval; the shearing stress is taken to be equal to the weight of the instrument head and needle divided by the average area of the submerged portion of the needle during that time interval.

Since the ASTM standard needle has a cylindrical shank 0.05 cm in radius, and a tip which is the frustrum of a cone whose altitude is 0.43 cm, whose smaller base radius is 0.015 cm and whose larger base radius is 0.05 cm, the solution of our data falls into two categories:

Case I

If all penetrations are greater than 43

$$a=\frac{7.854\times10^{-5}(P_f-P_i)}{t}, \qquad b=\frac{0.098\times10^4 W}{0.001571(P_f+P_i)-0.0521},$$

when t = time interval in seconds,
P_f = final penetration for interval t, in decimms.,
P_i = initial penetration for interval t, in decimms.,
W = weight of instrument head and needle in grams.

Case II

If all penetrations are less than 43, $a=V/t$ and $b=980W/A$ when V is the volume and A the area of the submerged portion of the frustrum; these values are found from graphs of penetration *versus* area and penetration *versus* volume of the needle, prepared from the formulae for the area and volume of a frustrum.

DISCUSSION

Curve *a* in Fig. 1 is for a 267° m.p. steam refined asphalt so hard and brittle that it spalls violently when struck with a hammer; the calculations and data are given in Table I. Its yield value is 43,000,000 dynes/cm² and its mobility is 1.0×10^{-14}, reciprocal poises. Curve *g* is for a very heavy cut-back road oil; the data are given in Table II.

TABLE I. *W = 350 grams. Solved in Case II.*

Total time	Total pen.	Volume P_f	Volume P_i	Change in vol.	10^7a	Average area	$10^{-7}b$
20	2.5						
80	4.5	0.000031	0.000012	0.000019	3.3	0.0044	17.7
200	5.75	.000049	.000031	.000018	1.3	.0063	5.5
400	6.25	.000054	.000049	.000005	.25	.0076	4.5

[1] D. V. Gregory, G. M. Rassweiler and K. C. Lampert, J. Rheology 1, 30 (1929).
[2] ASTM Standard Test Method D5–25.

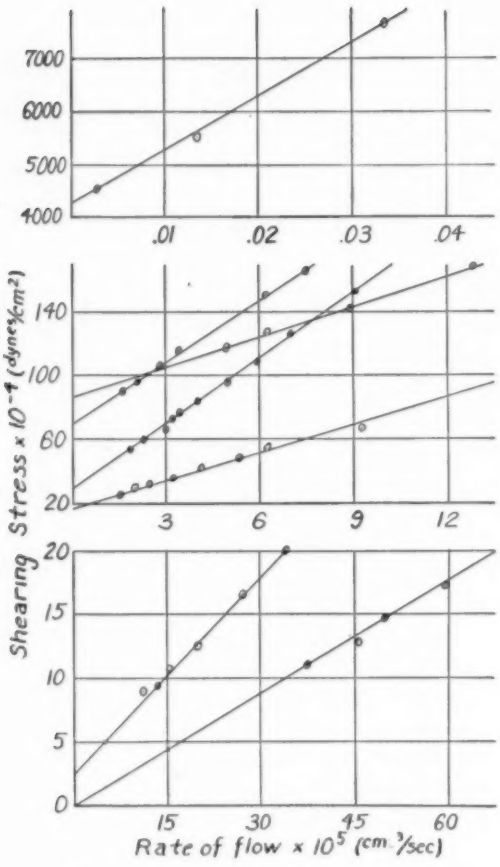

FIG. 1. Asphalt flow under penetration needle.

Curve *b* is for an air-blown material melting at 182°F; *c* is a steam-refined product of 144 m.p. and 22 pen.; *d* is a Mexican asphalt of 181 m.p. and 25 pen.; *e* is an air-blown material of 147 m.p. and 41 pen.; *f* is a steam-refined material of 108 m.p. punched with a load $W=50$ grams.

Data from the first time interval on hard asphalts, and from the first several intervals on soft asphalts has been found to be erratic; this may be due to the error, negligible with other intervals, introduced by the use of shearing stresses calculated for the average penetration during the interval rather than for the penetration for median time. Opposed to the findings of Traxler, Pittman and Burns[3] that the first penetration does not correlate well with apparent viscosity are the findings of Saal and of Pfeiffer and van Doormaal[4] who ingeniously use the deviation found between viscosities based on the first penetration and predicted viscosities as a measure of the plasticity of the asphalt. No other systematic errors have been observed in the 70 determinations so far made; the accuracy of the method apparently depends upon the skill of the operator. Errors attributable to elasticity, thixotropy or syneresis have not been detected. Rate of penetration in itself apparently makes little difference; penetrations of from 8 decimms., down to 0.0025 decimms. have yielded satisfactory data. In general, this method probably gives more accurate yield values than mobilities, since this variable is less sensitive to temperature, shape of the surface at the point of punching, and age of the material; with care either variable can be checked to within a few percent.

[3] R. N. Traxler, C. U. Pittman and F. B. Burns, Physics **6**, 58 (1935).

[4] J. P. Pfeiffer and P. M. van Doormaal, J. Inst. Petrol. Tech. **22**, 414 (1936).

RESULTS

The following data show general agreement between the results obtained by this method and plastometric theory:[5]

A. Effect of surface

A sample with a strongly concave surface was punched both at the horizontal center and near the edge of the can where the surface sloped at about 45 degrees:

At center, $f=450{,}000$ dynes/cm²; $m=4.38\times10^{-11}$ recip. poises. At edge, $f=430{,}000$ dynes/cm² while the value of m was 8.4×10^{-11}. The yield value, being independent of the rate of flow, is practically unaffected; the mobility is almost doubled.

B. Effect of temperature

An air-blown asphalt melting at 182 degrees F was punched at 32,54,100 and 118 degrees F. As shown in Fig. 2, minus log m varies linearly with the temperature—a relationship expected from Traxler and Schweyer's finding of a linear correlation between log viscosity and temperature;[6] if $f=0$, minus log m equals log viscosity. That log penetration varies linearly with the temperature has already been reported by Pfeiffer and van Doormaal;[4]

TABLE II. *Solved in Case I. $W=100$ grams.*

Total time	Total pen.	10^5a	$10^{-4}b$	Viscosity $\times10^{-8}$
5	127			
10	176	77	23.5	3.05
15	214	60	17.5	2.92
20	245	49	14.5	2.95
25	274	45	13	2.89
30	298	38	11.5	3.03

TABLE III.

m.p. (R & B)	MOBILITY at 77°	PREDICTED m.p.	DIFFERENCE
Charge stock I. $s=.0292$. $r=7.728$.			
120	1.03×10^{-9}	120	0
131	5.25×10^{-10}	130	−1
142	2.56×10^{-10}	140.5	−1.5
145	1.95×10^{-10}	145	0
153	1.12×10^{-10}	153	0
Charge Stock II. $s=0.0238$. $r=8.14$.			
126	6.6×10^{-10}	125	−1
143	2.1×10^{-10}	142	−1
186	2.18×10^{-11}	183	−3
216	2.74×10^{-12}	216	0
250	4.25×10^{-13}	255	+5

[5] E. C. Bingham, *Fluidity and Plasticity* (1922), Chaps. VII and VIII.

[6] R. N. Traxler and H. E. Schweyer, Physics **7**, 67 (1936).

since the pen. is a function of f and $1/m$, $\log f$ is expected to show a similar variation with the temperature. That the yield value is much less affected by temperature than the mobility is confirmed by Pfeiffer and van Doormaal's finding that asphalts of high plasticity have lower temperature susceptibility.

C. Effect of pressure

A particular D grade asphalt was punched twice with different loads:

$W = 200$ grams, viscosity $= 3.20 \times 10^9$ poises,
$W = 100$ grams, viscosity $= 3.28 \times 10^9$ poises.

Corresponding viscosities calculated from the first penetration only are respectively 5.08×10^9 and 9.41×10^9 poises.

D. Yield value and asphaltene content

It has been mentioned by Thurston and Knowles that the asphaltene content of an asphalt is responsible for its plastic properties.[7] The asphaltene content of four air-blown asphalts from the same crude stock was determined as the percent of the asphalt insoluble in 88° Be petroleum ether; the correlation found between yield value and asphaltene content, shown in Fig. 3, is the same in type as that found by Bingham[5] for the dispersed phase of other plastic solids.

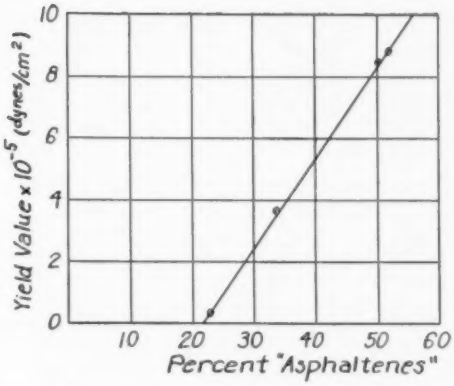

FIG. 3. Dependence of yield valve upon solids content.

E. Effect of age after casting

In general, asphalts gain in yield value and lose in mobility on aging after pouring: 159° m.p. air-blown. Fresh $f = 284{,}000$ dynes/cm²; $m = 7.88 \times 10^{-11}$ recip. poises. After 8 days, $f = 313{,}000$ dynes/cm²; $m = 4.80 \times 10^{-11}$ recip. poises. After 60 days $f = 337{,}000$ dynes/cm²; $m = 3.95 \times 10^{-11}$ recip. poises.

F. Correlation between melting point and mobility

With the two crude stocks so far tried, it has proved possible to predict the Ring-and-Ball melting points of the finished air-blown asphalts from their mobilities at 77 degrees F:

m.p. $= (-\log m - r)/s + 77$ when s = the increase in the value of minus log m per degree rise in m.p. and r = log of the hypothetical viscosity of an asphalt melting at 77 degrees. Since the relationship holds good only with materials melting above 120°F., the value of r is purely hypothetical. Data for the two charge stocks, given in Table III, show that for small ranges in melting point, the equation is precise within the limits of experimental error; over large m.p. ranges, the substitution of glycerin for water for high m.p. in the melting point determination may be an upsetting factor.

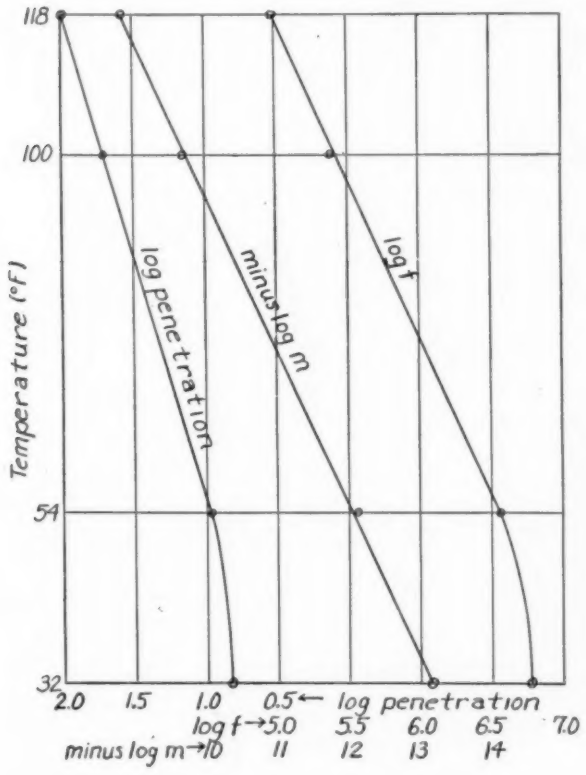

FIG. 2. Effect of temperature on an asphalt's properties.

ACKNOWLEDGMENTS

The author wishes to express his thanks to Dr. E. C. Bingham for help in preparing this report, and to his associates at Casmite Company, Mr. A. C. Young, Mr. C. F. Allee and Mr. C. Hoon, for their cooperation.

[7] R. R. Thurston and E. C. Knowles, Ind. and Eng. Chem. **28**, 88 (1936).

Effect of Temperature on Stretched Rubber Bands*

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(Received December 21, 1936)

Specimens of each of two vulcanized rubber compounds of known composition were subjected to various constant gravity loads in a controlled temperature chamber for several months. The lengths of the stretched bands were observed from time to time while the temperature was altered in gradual steps within a range of about thirty degrees centigrade centering around room temperature. A projection method for observing small changes in length was devised for the experiment. The contraction of stretched rubber with rise in temperature, first observed about the middle of the eighteenth century, was verified, as also was its positive elongation when unstretched or when but slightly stretched. The present experiment shows that the expansion or contraction for each specimen was constant when measured in cm per degree temperature change, but not when measured in percent of the prevailing length of the band. That is, the thermal elongation was most simply expressed when considered independent of the after effect or drift. For molded bands originally 1 mm in thickness, 4 mm in width, and 10 cm in mean circumference, and with loads ranging from 30 to 836 grams producing elongations up to about 700 percent, the rates of expansion were found to lie between plus 0.0037 and minus 0.0550 cm per degree. The relation between the rate of thermal elongation, $dl/d\theta$, and the stretching load, w, was found to be linear within the limits of the experiment, and the constants c and d in the empirical equation,

$$dl/d\theta = cw + d$$

have been evaluated, the former being negative in sign. The critical tensions for which there appeared no thermal change in length were 63 and 97 grams, respectively, for the particular shapes and compositions used. These tensions are equivalent to critical stresses of 7.72 and 11.9×10^5 dynes/cm² (based on the cross section of the unstretched band) or of 11.2 and 17.2 lb./in.², respectively, for the two compounds. Extrapolation by placing $w = 0$ does not seem justifiable because when the resulting rate is divided by one-half the circumference of the bands the magnitude of the coefficients of linear expansion turn out to be several times the generally accepted values based on volumetric determinations.

INTRODUCTION

THAT stretched rubber has a negative thermal expansion, an observation probably first made by Joule in 1858,[1] had been pointed out by most of the physics text books of previous generations as one of the surprising phenomena of nature, and upon duly impressed audiences of former decades it seems that there was imposed many a lecture demonstration of this effect by the passage of steam through a piece of rubber tubing stretched by a weight and provided with a suitable indicator of length. In the experiment now reported the lengths of rubber bands, under various constant tensions and of known composition, were observed as a function of temperature.

METHOD OF MEASUREMENT OF LENGTHS

Variations in the length of the bands being quite small for temperature changes of the order of ten or twenty degrees, a special method of observation was required. For this purpose a centimeter scale S, shown in Fig. 1, was imposed between the specimen B and its load L. Strictly parallel with scale S was planted a fixed scale F. Horizontally in front of the specimen scales was stretched a hair wire W, kept taut by a load and pulley at either end. A line of sight was established by the selection of a convenient mark on the movable scale, and the projection of the hair line through this mark to the fixed scale. Sighting was successfully done through the double glass front of the cabinet in which the specimens were suspended, there being sufficient depth of focus for the estimation of tenths of millimeters on the fixed scale. Each specimen scale was calibrated by the direct method of sighting on the fixed scale from two or more chosen S marks (m, $m \pm 1$, etc.), with S stationary. The projection ratio k was then defined as

$$k = F_m - F_{m-1}, \tag{1}$$

where the F's represent the respective projected readings. By geometric considerations it follows that F_m, a function of the length, l, of the specimen, is linearly related to l and may therefore be expressed as

$$F_m = kl + h, \tag{2}$$

where h is constant.

In cases of pronounced change in length of the specimen, the observed readings had to be made from several successive reference marks on scale S. These respective readings on the fixed scale were reduced to those for a common reference mark, m, on the individual specimen scale by the geometric relation,

$$F_m = F_x + (m - x)k, \tag{3}$$

where F_m represents the projected readings from mark m; where x represents centimeter marks on the S scale more conveniently observed at later times, F_x their respective projections; and where k is the above defined projection factor. These relationships are illustrated in Fig. 2. It should be stated that x may have been greater than m or less than m depending upon whether the band was expanding or contracting. The projection factors gave a reading magnification ranging from two to twenty-fold, the magnitude of which depended upon the position of the line of motion of the specimen and its

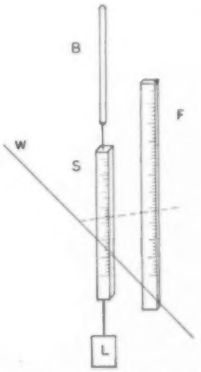

FIG. 1. Rubber band B stretched by load L. Scale S moves with the changing length of the band but scale F and horizontal hair wire W are fixed. Projected reading on F is obtained by moving eye up and down until wire W coincides with chosen reference mark on S.

* Paper presented at the eighth annual meeting of the Society of Rheology, New York, N. Y., October 31, 1936.

[1] J. P. Joule, Phil. Trans. Roy. Soc. London **A149**, 91 (especially page 106) (1859).

scale edge with respect to the fixed scale and the fixed wire.

Method of Altering Temperature

Heat was supplied to the Celotex insulated chamber electrically by means of a loosely coiled Nichrome resistance, and was distributed quite uniformly throughout the volume of the chamber by a gentle air current set up by a small fan driven by a clock motor. The temperature was varied in steps at conveniently chosen times by varying the electric current, and was evaluated by two tenth-degree thermometers so placed that a temperature change of one-hundredth degree could be noted from the outside of the cabinet. The temperature extremes of the experiment were 16 and 48 degrees C though no single set of observations had this wide range.

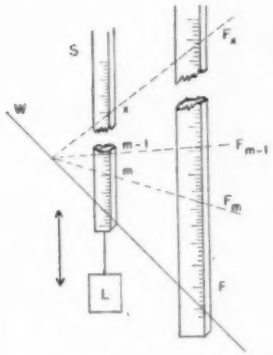

Fig. 2. Projection method of reading motion of scale S on fixed scale F. Projection factor $k=F_m-F_{m-1}$ (with S temporarily fixed). Reduction of observed reading F_x (as mark x moves into vicinity of the hair wire) to that, F_m, for standard reference mark m is made by the relation $F_m=F_x+(m-x)k$ $x \lessgtr m$.

Data were obtained by increasing or decreasing, in discrete steps, the heat delivered to the specimen chamber and by allowing at least five minutes of stability of the thermometers before readings of the respective lengths were undertaken. Occasionally an exception to this method was made by the taking of readings during a slow natural cooling of the chamber. By one or the other of these methods a set of several readings on each of a score or more series of temperature changes was taken at selected times over a period of five months.

The Specimens

The specimen bands were vulcanized in ring shaped molds and measured very approximately 1 mm in thickness, 4 mm in width, and 99 mm in mean circumference, making the unstretched length of the bands about 5.0 cm. They were prepared from two distinct rubber compounds, one of which was vulcanized by sulphur alone, and the other by tetramethyl-thiuram disulphide without the use of sulphur as such. Both compounds were of the "pure gum" type, i.e., they contained no significant amounts of fillers. Their compositions are shown in Table I. Compound No. 1 was comparatively fresh but compound No. 2 had aged for more than a year in the dark. The specimens were furnished by courtesy of the Rubber Division of the National Bureau of Standards.

Table I. *Composition of the specimens. Ingredients are given in parts by weight.*

	No. 1	No. 2
Ingredient		
Rubber	100.0	100.0
Sulphur	7.5	—
Tetramethylthiuramdisulphide	—	3.0
Stearic Acid	—	1.0
Zinc Oxide	—	2.0
Vulcanization		
Temperature	145°C	135°C
Time	150 min.	30 min.

The bands were given their respective loads isothermally and were allowed to drift for a week before a series of observations was undertaken. During the experiment the bands were shielded from strong direct light. For each compound six different stretching tensions were used, the magnitudes of which ranged from 30 grams, the weight of the scale, to 836 grams. It was necessary to use some weight in order to deform the ring shape into that of a band with parallel sides. The stresses, based on original cross section, ranged from 3.7×10^5 to 1.0×10^7 dynes per sq. cm, while the maximum elongation noted was about 700 percent. The specimens were in contact at either end with straight polished brass hooks whose diameters were comparable to the thickness of the bands.

Results: I. Relation Between Length and Temperature

When the observed projection readings for a given specimen, reduced when necessary to those for a standard reference mark as above explained, had been plotted against their corresponding temperatures the resulting graph showed a family of straight lines. Each of these lines represented one of the series of temperature changes. A representative plot is shown in Fig. 3, which represents data for specimen *I*, compound No. 2, tension 529 grams.

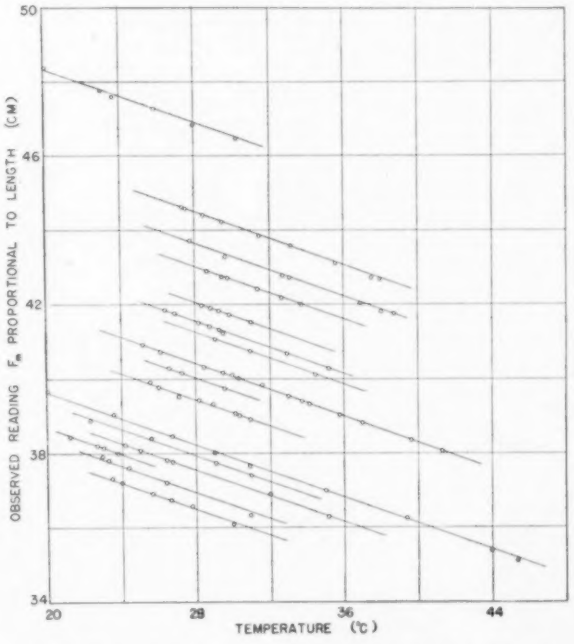

Fig. 3. Observed readings, F_m, in cm (proportional to length) for rubber band specimen *I*, compound No. 2, under constant tension of 529 grams, as a function of temperature, θ, in degrees centigrade. Data for the lowest curve were taken on the sixth day; those for the uppermost on the 158th day. The common slope is $a=-0.177$.

TABLE II. *Experimental data and rates of thermal elongation for vulcanized rubber bands with unstretched lengths of 5 cm.*

Compound	Specimen	Load in grams w	Projection factor k	Slope $a\times10^3$	Elongation in cm $\times10^4$ per degree C		
					$y=a/k$	$y=cw+d$	Diff.
No. 1	D	834	20.2	−111.1	−550	−551	1
	C	529	6.84	− 22.7	−333	−333	0
	L	432	12.1	− 32.1	−265	−264	1
	K	279	2.25	− 3.50	−155	−155	0
	N	83	9.96	− 1.44	− 14	− 14	0
		63*				0	
	S	30	7.4	+ 2.58	+ 35	+ 24	11
		0				+ 45**	
No. 2	J	836	12.4	− 51.0	−411	−411	0
	I	529	7.4	− 17.7	−239	−240	1
	Q	279	2.2	− 2.28	−104	−101	3
		97*				0	
	T	82	10.5	+ 2.00	+ 19	+ 9	10
	X	30	7.3	+ 2.70	+ 37	+ 37	0
		0				+ 54**	

* Interpolated value. ** Extrapolated value.

It could be discerned that the lines for a given specimen tended to bear the same slope. This indicated a common rate of expansion for a given load and specimen, independent of the prevailing length of the band. The lowest line in Fig. 3 gives data taken on the sixth day while the topmost line is for those recorded on the 158th day. Evidently the parameter of this family of lines was a function of time, accounted for by the phenomenon of drift[2] or after effect in stressed rubber. Consequently when F_m represented the observed readings of length (reduced from F_x when necessary), and when θ represented the corresponding temperatures, it was apparent from the graphs that for each specimen

$$F_m=a\theta+b, \tag{4}$$

where a is the common slope of the family of lines and where b, like h in Eq. (2), is a constant depending for its magnitude on the orientation of the fixed scale. Included in the make up of this constant would be: (*a*) the scale reading corresponding to the original length of the specimen, (*b*) the increment in length due to the placing of the load, and (*c*) the drift. While the drift is doubtless a function of temperature its significance in this respect in the present treatise is considered negligible.

Setting the right hand members of Eqs. (2) and (4) equal to each other,

$$kl+h=a\theta+b,$$

differentiating with respect to θ, and calling the derivative y for short gives

$$y\equiv dl/d\theta=a/k. \tag{5}$$

Thus it appears that for a given stretched specimen the thermal rate of elongation is constant. Experimental values of y obtained in this manner are shown in Table II. It is to be noted, however, that this constant rate is not one and the same thing as the familiar coefficient of linear expansion. It could only be interpreted as such when the original stretched or unstretched length, unaltered by drift, is read into the definition of the coeficient, and it is questionable whether this should be done.

RESULTS: II. RELATION BETWEEN RATE OF ELONGATION AND LOAD

Table II shows that the slopes for the higher stresses were negative while those for the lightest loads were positive. That is to say, the more highly loaded specimens contracted with increase in temperature while the least loaded bands became slightly longer as the temperature increased. The converse effect was just as definite. A lowering of temperature was accompanied by elongation for the more severely stretched bands, and by contraction for the very low or nearly zero stretches. (A minute hysteresis exists but its effect is beyond the significant figures of this paper.) The value of y was therefore found to be dependent upon the magnitude of the stretching load. At least within the time, temperature, and stress ranges involved in the experiment this dependence of the expansion rate upon the load seemed to be linear. This rate as a function of the load is disclosed in Fig. 4. From Fig. 4 it is apparent that

$$y=cw+d, \tag{6}$$

where w is the load in grams. The slope, c, is negative in sign. Experimental values for the constants, c and d are given in Table III. Specimen S was discarded in this computation because of its unusual deviation. Values of the rate of elongation as computed by Eq. (6) are entered in Table II along with those obtained individually by Eq. (5), and the difference between the two computations is pointed out. It should be stated that in cases of the low load specimens, N, S, T, and X, the total change in length during the four or five month period of the experiment

TABLE III. *Constants in Eq. (6) $y=cw+d$, and extrapolated values (with $w=0$) for α, the thermal coefficient of linear expansion.*

COMPOUND	c	d	α
No. 1	-71.5×10^{-6}	44.6×10^{-4}	8.92×10^{-4}
No. 2	−55.5	53.6	10.72

[2] M. L. Braun, Physics **7**, 421 (1936).

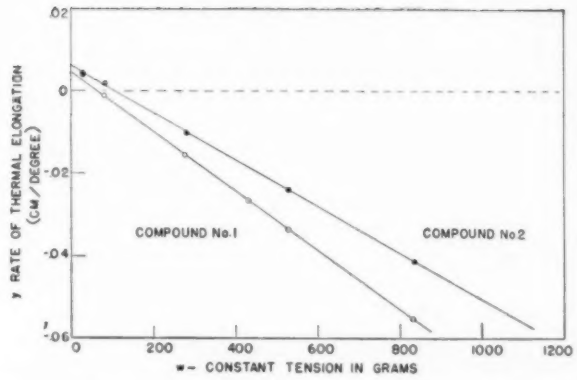

FIG. 4. Temperature rate of change in length of stretched rubber bands, y, in cm per degree, as a function of their constant tension, w, in grams. For compound No. 1, $y=(-71.5\,w+0.446)\times10^{-6}$. For compound No. 2, $y=(-55.5\,w+0.536)\times10^{-6}$.

amounted in no individual case to as much as 2 mm, and since most of this change was due to drift it was difficult to secure a great deal of consistency in the observations for these particular loads. The values of a for specimens S and T, for example, were obtained by selection of six or eight of the most consistent series. Experimental readings for a few of the other series were even of opposite sign.

Eq. (6) gave intercepts of plus 0.00446 and plus 0.00536 cm per degree, respectively, as the rates for zero load. Dividing these figures by one-half the circumference of the bands gave computed values of 0.000892 and 0.001072, respectively, for the thermal coefficients of linear expansions for the compositions used. These values are about four times greater than expectations based on determinations of the coefficients of volume expansion by Bekkedahl[3] and others.[4] While the present intercepts may or may not be applicable to the perimeter of bands lying on a frictionless table, the interpolated values of 63 and nearly 100 grams for zero change in length with temperature are reasonably demonstrable. For the two compounds these critical tensions were about 6.18 and 9.51×10^4 dynes, while the corresponding stresses, for which $y=0$, amounted, respectively, to 7.72 and 11.9×10^5 dynes/cm^2 (0.79 and 1.21 kg/cm^2, or 11.2 and 17.2 lb./in.^{2}). In the computations of these stresses the change in cross-sectional area for the comparatively light loads involved was considered negligible.

In conclusion the author wishes to acknowledge encouragement given him to continue experimental studies on the physical properties of rubber by Dr. A. T. McPherson, Director of the Rubber Division of the National Bureau of Standards, and the interested assistance of several of his own students, especially of Mr. Arthur C. Menius, Jr.

[3] N. Bekkedahl, Nat. Bur. Stand. J. Research **13**, 411 (1934).
[4] Curtis, McPherson and Scott, Nat. Bur. Stand. Sci. Paper 560 (1927).

The Daylight Variation of Signal Strength

R. C. COLWELL AND A. W. FRIEND
West Virginia University, Morgantown, West Virginia

(Received December 4, 1936)

A rapidly moving recording galvanometer (Micromax) is used to maintain balance in a Wheatstone bridge. Received radio signals unbalance this bridge, so that an automatic record of voltage signal strength is taken. On daylight signals from nearby stations, the signals fluctuate rapidly during each day and night signals show a relationship dependent upon the barometric pressure. The latter observations agree with those made by Martyn in Australia. As autumn approached in 1936 every station recorded showed an increase of signal strength during the daylight hours.

THE general theory of the propagation of long radio waves amply substantiated by experiment indicates that during the day, the so-called ground wave is alone operative; at night the space wave is reflected from the ionosphere, causing an increase of intensity in the received signal and also producing the fading which is characteristic of night reception. During investigations made upon the signal intensity of KDKA at Morgantown, it was found that even in the daytime, there was a great variation from day to day. Preliminary observations made during the year 1934 are given in Table I. The records were taken on a superheterodyne working into the galvanometer of a Shaw recorder. The curves were taken for two hours each morning and afternoon. The average intensity was calculated by means of a planimeter. The receiver was kept at the same sensitivity throughout by the use of an accurate signal intensity oscillator. The values given in the table are relative values only and have no absolute significance. It appears from them, however, that the average signal was 6.58, the minimum 2.65 and the maximum 10.83. Hence, the strongest daylight signal received during the measurements was about four times the weakest signal and sixty percent stronger than the average.

It was decided to make more accurate observations upon the signals using a self-recording galvanometer. In Fig. 1 is shown the circuit diagram of the connections between

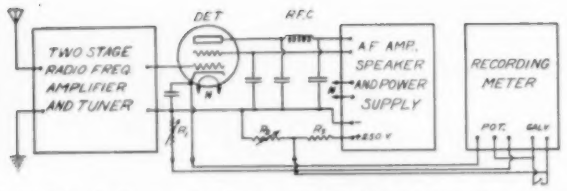

FIG. 1. Circuit diagram showing connection of signal strength recording galvanometer to the receiver.

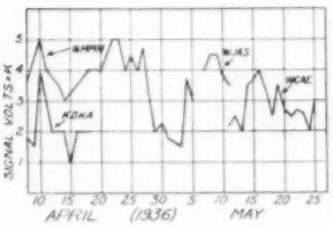

FIG. 2. Average daylight signal strengths from automatic recorder.

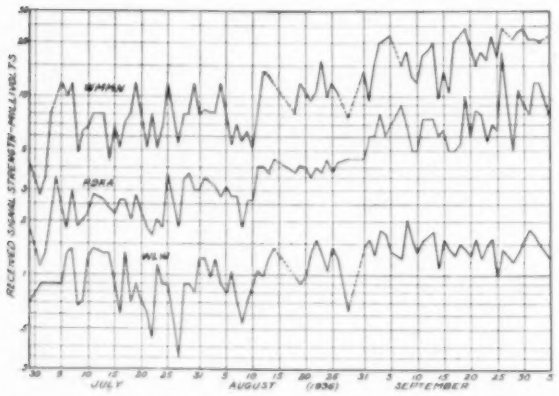

FIG. 3. Noon-day signal strengths recorded on calibrated attenuator of a superheterodyne receiver.

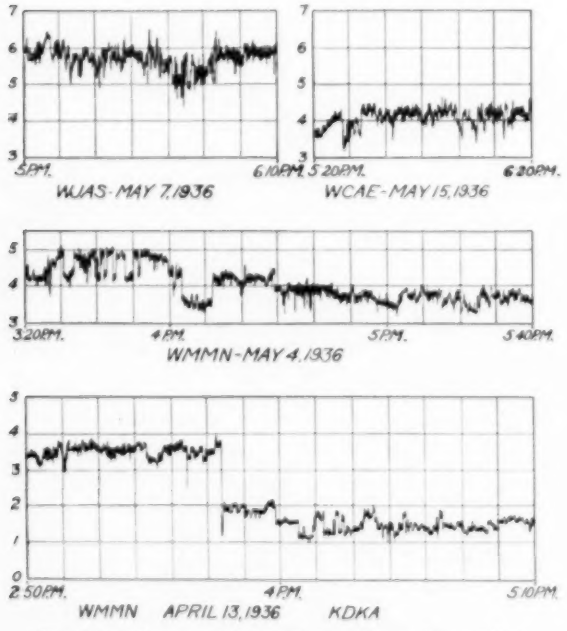

FIG. 4. Daytime recorded signal curves for various stations.

the tuned radiofrequency receiver and the recording meter. R_1 is the usual grid bias cathode resistor which forms the greater portion of one leg of a Wheatstone bridge circuit. This branch is completed by the meter potentiometer, which also completes the second side of the bridge formed mainly by the vacuum tube detector. The bridge is completed by resistors R_2 and R_3. The variations in signal strength produce an unbalance of the bridge circuit which is automatically rebalanced by the potentiometer circuit of the recording meter. The movements of the potentiometer contact are proportional to the voltage of the received signal and are traced on a chart by the recording pen. This instrument is much more rapid in following signal fluctuations than those formerly used. It shows the rapid daytime variations as well as the more intricate pattern of nighttime fading.

Contrary to the usual theory that the daytime signal is fairly constant, it was found that wide fluctuations took place from day to day. An extremely rapid type of daytime fading occurred in greatest magnitude on the signals of WMMN located only 20 km from the receiving station. This rapid fading was noticeable only if the recording needle moved fast enough to show fading of about one second. On slower recorders, the rapid fading could not be seen. In Fig. 2 curves are given showing the average daily strength for several nearby stations. The magnitude of the daily change is clearly indicated by these graphs. These records were compiled from a series of readings taken on the machine of Fig. 1. Later a series of readings was begun using a standard superheterodyne receiver with a calibrated attenuation control. Fig. 3 shows the record obtained for the three stations WMMN (distance 20 km), KDKA (150 km) and WLW (500 km) for the period from June 29 to October 5, 1936.

A steady increase of signal strength is noted as autumn

TABLE I. *Signal intensity of KDKA at Morgantown.*

DATE 1934	SIGNAL STRENGTH	WEATHER	TEMPERATURE	BAR. PRESSURE
March 28				
A.M.	2.65	Fair	25°	29.66
P.M.	2.35	"	40°	29.66
March 29				
A.M.	3.98	"	No record	29.72
P.M.	4.38	"	"	29.67
March 30				
A.M.	4.43	"	"	29.61
P.M.	2.73	"	"	29.57
April 24				
A.M.	6.30	Cloudy	65°	29.14
P.M.	7.50	"	78°	29.16
May 22	8.57	Fair	75°	29.10
23	10.83	"	66°	29.31
24	3.95	"	65°	29.21
25	8.38	"	55°	29.30
26	10.83	"	63°	29.37
June 1	9.67	"	75°	29.17
7	5.12	"	80°	29.32
8	8.58	"	77°	29.44
9	8.47	Rain	68°	29.30
12	9.72	"	65°	28.95
Average	6.58			

approaches, and superimposed upon this is a day to day fluctuation with maximum amplitude of about a four to one ratio. Fig. 4 shows several examples of the daytime fading consisting of the very rapid type combined with large changes of longer duration. Such a variation cannot very well be attributed to changes in the conductivity of the soil. A more likely explanation lies in the variations of the height and density of the *C* region of the lower ionosphere which lies at a height of from 1 to 12 km.[1] It has been found that when a large thunderstorm area passes between Morgantown and any nearby sending station, a drop in signal strength will occur. During severe magnetic disturbances when the *C* region is always low, the received signals from nearby stations are weak. These observations seem to connect daylight fading with changes in the lower ionosphere.

Some automatic records were also made of the day to night changes. The results for a low pressure area and a high pressure area are shown in Figs. 5 and 6. It is quite clear from these that when a low pressure area is approaching from the northwest, the night signal from KDKA is much higher than the day.[2] A high pressure area is indicated when the night signal does not rise much above the day signal.

Martyn[3] has summarized his findings regarding the ionization in the *E* region as follows: "A very close direct correlation is evident between the average nighttime ionization density in the *E* layer and the barometric pressure at ground level measured at a time ranging from 12 to 36 hours after the ionization observation. For example, if on any night the average ionization density is greater than on the preceding night, then the barometer invariably rises within the time interval mentioned.

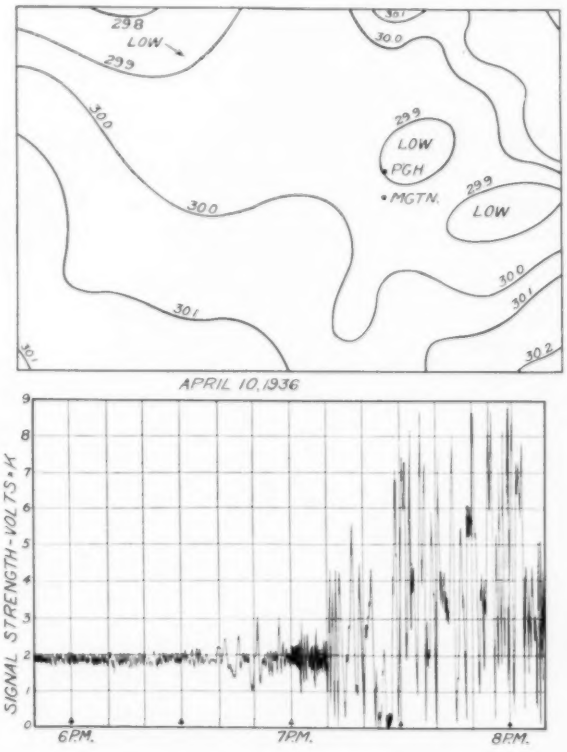

FIG. 5. Low pressure area approaching from northwest causing severe evening fading of KDKA signals.

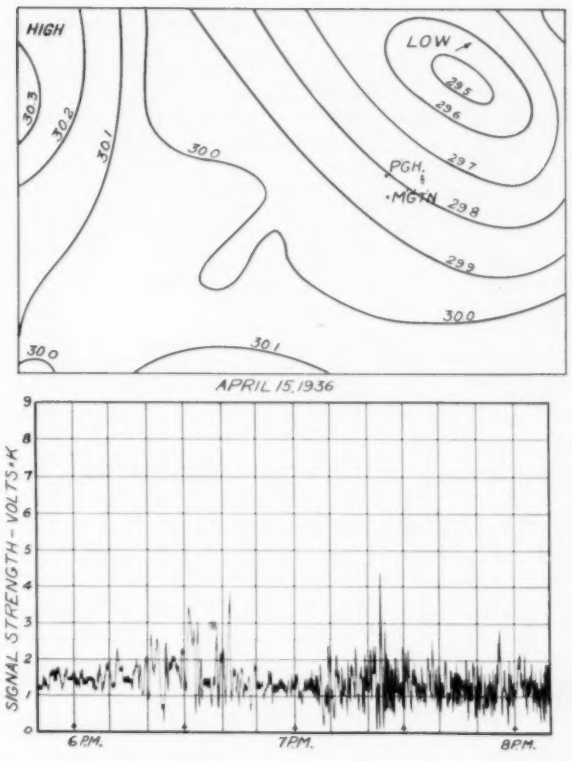

FIG. 6. High pressure area approaching from northwest causing reduced evening fading of KDKA signals.

"The results obtained in the first series of experiments are in complete accord with those of Colwell though the explanation offered differs considerably from his. Thus Colwell considers that the *E* layer is concentrated in the regions of low pressure, resulting in a stronger post-sunset signal. It is much more probable that a stronger night signal would result from a decreased intensity of ionization in the absorbing portion of the *E* layer."

If the newly discovered *D* and *C* regions of the ionosphere[4] are regarded as the absorbing layers of the *E* region, the observations given in this paper are in agreement with Martyn's theory. During the summer months, it has been impossible to measure the *D* layer with accuracy as it was found infrequently. On the other hand observations upon the *C* region show that it rises in low pressure areas and falls in high pressure areas. Since the rising gases expand, the number of ions per cc will be less when the *C* region is high above the ground, hence its absorbing power will be reduced.

[1] Colwell and Friend, Nature **137**, 782 (1936); Watson-Watt, Bainbridge-Bell, Wilkins and Bowen, Nature **137**, 866 (1936); Rakshit and Bhar, Nature **138**, 283 (1936). Colwell and Friend, Phys. Rev. **50**, 632 (1936).

[2] R. C. Colwell, Proc. I.R.E. **21**, 721 (1933).

[3] D. F., Martyn, Nature **133**, 294 (1934).

[4] E. V., Appleton, Proc. Roy. Soc. **A126**, 542 (1930); Mitra, S. K. and Syam, P., Nature **135**, 953 (1935); cf. reference 1.

The Limiting Problem of Internal Ballistics

In a recent paper[1] R. H. Kent stated that he was pointing out an important error in my solution[2] of the equation of motion of the propellant in a gun. The reason given is that, apparently without being aware of it, I assumed that the initial density is uniform, and obtained a solution inconsistent with that assumption. My view is that Mr. Kent's objection is purely verbal and that no error has been made.

May I first say that I am grateful to Mr. Kent for two points in his paper? Firstly, he has confirmed the numerical accuracy of my expansions

$$R=1-\frac{1}{2}\epsilon+\frac{75}{264}\epsilon^2-\frac{14{,}913}{87{,}120}\epsilon^3+\cdots, \qquad \alpha=\frac{1}{3}-\frac{2}{55}\epsilon+\cdots,$$

when $\gamma=11/9$, into the first of which a numerical slip might easily have crept. Secondly, he has obtained the same expansions, up to the order given, for covolumes 0 and 1. With this hint I now show that the problem is in all essentials independent of the covolume. My paper contains, on p. 222, expressions like $1/\rho_0-1$ in which the 1 is the covolume. If this is replaced by an arbitrary number, only slight changes are needed in six of the earlier equations, and the later equations, from $A=\epsilon/\gamma bf(b)\{f^1(b)\}^\gamma$ onwards, are unchanged. It is unnecessary to support the agreement up to a certain order, as Mr. Kent does, by appealing to the fact that the gas approaches one of no covolume as it expands. The theorem is an exact one.

On the main point, let me first show that the agreement of Mr. Kent's equations and mine is not merely approximate (that is, to terms in ϵ^3 only) but complete. The notation of the two papers is not always the same. What I called w and a[2] Mr. Kent calls x and $1/a$. Combining his equations

$$a=[(\gamma-1)/\gamma][\rho_0(0)/2M](1-a)^{\gamma/(\gamma-1)} \text{ and}$$

$$C=\rho_0(0)\int_0^1(1-ax^2)^{1/(\gamma-1)}dx,$$

we have his equation to determine a laid out explicitly as

$$\int_0^1(1-ax^2)^{1/(\gamma-1)}dx=\frac{(\gamma-1)\epsilon}{2\gamma a}(1-a)^{\gamma/(\gamma-1)}.$$

Mr. Kent, as he can without loss of generality, takes the column of gas to be of unit length at the moment considered. This means $c=1$ in my notation, and my paper contains the equation

$$\int_0^c(a^2-w^2)^{1/(\gamma-1)}dw=\frac{(\gamma-1)\epsilon}{2\gamma c}(a^2-c^2)^{\gamma/(\gamma-1)}$$

to determine c/a. The equations are identical, and the distribution of pressure is identical too.

It appears to me, therefore, that Mr. Kent's solution is physically the same as mine, and this is why I conclude that his objection is verbal. All he has done is to obtain my results by a different method. I cannot be sure what Mr. Kent had in mind when he wrote that I was apparently unaware of having made the assumption of uniform initial density. I was quite aware of having made it. What I think Mr. Kent has not realized is that the assumption is harmless and does not imply that the state of uniform density is one of the states covered by my solution. Something must be done, when using the so-called Lagrangian equations in hydrodynamics, to identify the particle whose equations of motion are being written down. It is only *convenient* to suppose that it was at some specified point at a specified time. If the motion tends to a limiting form, we do not in general arrive at the initial point by extrapolating the limiting motion backwards. Suppose for example that a single particle falls under gravity in a resisting medium. It may be convenient to say that it was originally at rest at height z_0, the time being t_0. But if we take the limiting motion and suppose it reversed until time t_0, we do not reach the height z_0. The state of rest is separated from the limiting motion by other solutions of the same differential equation. This does not mean that the special solution which gives the limiting velocity is wrong. This is what I had in mind when writing on p. 224 that "the previous definition in terms of the work done from an initial state of uniform density is not convenient, as this state is not one of the previous states of the gas." Had I thought it possible to overlook the distinction between the initial state and the limiting state considered as existing at some earlier time, I should have explained it more clearly. As it is, I think this passage probably led Mr. Kent to think that I had done something without knowing it.

F. B. Pidduck

Corpus Christi College,
Oxford, England,
January 12, 1937.

[1] Physics **7**, 319 (1936).
[2] Phil. Trans. Roy. Soc. London **A222**, 222 (1921).

Special Solutions for the Motion of the Powder Gas (Comments on Mr. Pidduck's Note)

I apologize to Mr. Pidduck for having erroneously assumed that he was unaware of his assumption of constant initial density.

I am happy to learn that my expression for R for a gas of no covolume is identical with the one he published many years ago for a gas having a covolume. However, Mr. Pidduck's arguments fail to convince me that his solution and mine are physically equivalent, even though the results for R are identical. According to Mr. Pidduck's solution,[1] the relation between y and x is

$$y=\rho_0 x+w\varphi(t),$$

in which w is given by

$$\int_0^w(a^2-w^2)^{1/(\gamma-1)}dw=\frac{(\gamma-1)\epsilon}{2\gamma c}(a^2-c^2)^{\gamma/(\gamma-1)}\frac{x}{b}.$$

According to my solution[2] for the perfect gas, we have simply,

$$y=x\varphi(t).$$

I should like to call attention to Mr. Pidduck's equation on page 222,[1] an equation which he calls "the general hydrodynamical equation"

$$\rho_0\frac{\partial^2 y}{\partial t^2}=p_0\left(\frac{1}{\rho_0}-1\right)^\gamma\gamma\left(\frac{1}{\rho_0}\frac{\partial y}{\partial y_0}-1\right)^{-\gamma-1}\frac{1}{\rho_0}\frac{\partial^2 y}{\partial y_0^2}. \qquad (1)$$

[1] Phil. Trans. Roy. Soc. London **A222**, 222 (1922).
[2] Physics **7**, 319 (1936).

This equation, also, is obviously based on the assumption that ρ_0 is a constant, otherwise it would contain terms involving $\partial\rho_0/\partial y_0$, etc. If this is correct, it is hardly accurate to call the equation a "general" one.

Mr. Pidduck obtains a solution of the Eq. (1) which, according to his own statement, is inconsistent with the assumption of a constant initial density. The solution is therefore inconsistent with the assumption on which the equation is derived. Yet it purports to be a solution of this equation.

I am not nearly so sure as Mr. Pidduck is that R is independent of the covolume. It appears from my paper that the solution for the case where there is no covolume is different in form from the solution where there is a covolume.

Whether the assumption of constant initial density is harmless or not, it is unnecessary. In my reading of Mr. Pidduck's paper, it caused me to doubt the validity of his interesting and important evaluation of Pidduck's ratio, R. I believe that my paper gives a somewhat more rigorous derivation.

R. H. KENT

Aberdeen Proving Ground, Maryland,
January 28, 1937.

There are large investments in silk products in America. If the industry languishes, the doctor should suggest research, and research is now being carried on with many new artificial fibers where the wood of the tree is used instead of the leaves—a perennial supply instead of seasonal. There is no great probability that the complicated machinery of the tiny silkworm was designed to produce from mulberry leaves a product so well fitted for every changing **human** *service that better products might not be found. One naturally looks for such discoveries to those experimenters who already know the applications and the limitations of existing products, so that each industry should be inquisitive.*

—W. R. WHITNEY

Here and There

Dr. L. O. Grondahl, of the Union Switch and Signal Company, Swissvale (Pittsburgh), Pennsylvania, has been promoted from director of research to director of research and engineering, effective last January 1st. He now has general charge of all engineering development in connection with the products and manufacturing processes of the company. Dr. Grondahl has been director of research since the organization of a research department by the company in 1920.

•

Dr. Lawrence Wade Bass has been appointed a member of the executive staff of Mellon Institute of Industrial Research, according to an announcement of that institution. Dr. Bass served as executive assistant at Mellon Institute during the period 1929–31 and is returning to that organization as assistant director after being connected with the Borden Company, New York, N. Y., for six years, first as assistant director of research, then, since 1932, as director of research. He will assume his duties at the Institute on January 1, 1937.

•

The American Society for Testing Materials announces an exhibit of testing apparatus and related equipment, at the Waldorf-Astoria Hotel, New York City, June 28–July 2. One of its purposes is to demonstrate important developments in the apparatus and instrument fields since the last exhibit two years ago. This exhibit is held in connection with the 1937 A. S. T. M. annual meeting.

•

On December 29, 1936, at the meeting of the American Association for the Advancement of Science, Atlantic City, New Jersey, Dr. Henry G. Knight, Chief of the Bureau of Chemistry and Soils, U. S. Department of Agriculture, gave the Fifteenth Annual Lecture of Sigma Xi on "Selenium and Its Relation to Soil, Plants, Animals and Public Health."

•

The Lancaster, Pennsylvania, branch of the A. A. A. S., the Pennsylvania Academy of Science, and the Pennsylvania Conference of College Physics Teachers will all meet at Lancaster, Pennsylvania, March 26 and 27. It is expected that the three groups will have separate sessions on Friday afternoon and joint sessions Friday evening and Saturday morning.

Symposium on Textiles

A symposium on the application of physics in the cotton textile industry will be held in Chapel Hill on February 20 on the occasion of the joint meeting of the American Physical Society and the Southeastern Section of this Society. The following program of invited papers will be presented:

The Structure of the Cotton Fiber, by Dr. Wanda K. Farr, Cellulose Laboratory, Boyce-Thompson Institute for Plant Research, Yonkers, New York.

Moisture in Textiles, by Dr. A. C. Walker, Bell Telephone Laboratories, 463 West Street, New York, New York.

The Place of Statistics in Textile Research, by John P. Elting, Kendall Mills, Paw Creek, North Carolina.

The Physical Basis of Color Measurement, by Dr. A. C. Hardy, Massachusetts Institute of Technology, Cambridge, Massachusetts.

•

The fifth annual meeting and dinner of the Institute of the Aeronautical Sciences was held Wednesday through Friday, January 27 to 29, at the Pupin Physics Laboratories, Columbia University. A complete series of short papers was presented in the following fields: structural problems in aircraft design; instruments, radio and radiometrograph; fluid mechanics; long range seaplane and airship problems; engines and fuels; meteorology; general aerodynamics; airplane stability and performance; flying properties and flight testing.

•

A research program on the "Drying of Textiles" that is being developed by the United States Institute for Textile Research, Inc., represents a new application of physics to certain problems in the textile field. Authentic information is sought in order to determine just how much and under what conditions textile fibers can be dried without injury. These studies are being carried out by a trained physicist.

•

The following quotation from the meeting of the Council of the American Chemical Society as published in *Industrial and Engineering Chemistry* will be of interest to physicists.

"Thomas Midgley, Jr., discussed the serious problem of how to retain in the universities a few of the most promising students who might become qualified as teachers, thus assuring in perpetuity adequately trained men for activity in various fields of industry. He suggested as one tentative plan that industry agree to pay into a trustee fund to be administered by a committee of the American Chemical Society, on which there would be a number of men representing industry, $1000 for each doctor of philosophy employed by the given industry. This fund would be used in

whatever way proved best to retain for a stated period in universities men of the caliber needed. Five industries have signified a willingness to participate in such a plan. The following resolution was unanimously adopted:

"Resolved, That the president be, and hereby is, instructed to appoint a committee to devise and put into operation a system that is mutually acceptable to industry and the Board of Directors of the American Chemical Society whereby industry may cooperate, through the medium of the American Chemical Society, to protect the future progress of chemical education in the United States."

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Anniversary of the Johns Hopkins School of Engineering

An address by Dr. Karl T. Compton, president of the Massachusetts Institute of Technology, at the sixty-first Commemoration Day Exercises of the Johns Hopkins University on February 22nd, will be the principal event in a three-day celebration of the twenty-fifth anniversary of the University's School of Engineering.

The opening of a diversified exhibit of technical appliances and processes on the evening of February 19th will inaugurate the anniversary program. On the twentieth, prominent research and industrial specialists as well as officials of the city and state will gather to discuss technical papers which will be presented by leading faculty members of the Hopkins School. Current investigations and instruction methods will be considered during this conference period. Dean J. B. Whitehead, one of the three founding professors of the school, and Professor W. B. Kouwenhoven will read papers on "Limitations of High Voltage Insulation" and "Electrical Accidents," respectively, at the technical sessions.

Professor Niels Bohr, director of the Institut für Theoretische Physik, Copenhagen, will deliver an A. R. L. Dohme Lecture on the afternoon of February 22nd. Professor Bohr's subject will be "The Problem of Causality in Atomic Theory."

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The following election of officers of the American Association for the Advancement of Science took place at the annual meeting in Atlantic City: President, George D. Birkhoff; Permanent Secretary, Forest R. Moulton; Vice President and Chairman Section B, Harvey Fletcher, Committeeman of Section B, Jesse W. Beams; and Secretary Section B, Henry A. Barton.

●

Dr. John T. Tate, Chairman of the Governing Board of the American Institute of Physics and Editor of the *Physical Review* and of the *Reviews of Modern Physics* was appointed Dean of the College of Science, Literature and The Arts of the University of Minnesota. He will take office officially July 1. Dr. Tate has been at Minnesota since 1916. He is well known for his fundamental work of electron impacts in gases. Besides editing the journals mentioned above, Dr. Tate was the first editor of the journal *Physics* and has done much to initiate the new *Journal of Applied Physics*.

The Optical Society of America will hold a meeting at Corning, New York, on Friday and Saturday, March 5 and 6. One of the features of this meeting is the opportunity offered, through the courtesy of the Corning Glass Works, to see at first hand many applications of optics in the glass industry and contributions of the glass industry to optics. The Corning Glass Works are one of the largest manufacturers of glass in the world. All interested are cordially invited to attend this meeting.

●

A joint meeting of the American Physical Society and the American association of Physics Teachers will be held at the University of North Carolina at Chapel Hill and at Duke University at Durham, N. C., February 19 and 20. Besides the symposium on textiles announced on the opposite page there will be a symposium on the teaching of physics with the following invited papers:

Some Aspects of Physics Teaching in the South, by DEAN L. L. HENDREN, University of Georgia, Athens, Georgia.

What College Physics Expects of Pre-College Education, by THOMAS D. COPE, University of Pennsylvania, Philadelphia, Pa.

Graduate Training for a Career in Physics, by PROFESSOR G. P. HARNWELL, Princeton University, Princeton, N. J.

There will also be a symposium on nuclear physics for which the following papers have been arranged:

Nuclear Reactions and Angular Momentum, by HANS A. BETHE, Cornell University, Ithaca, N. Y.

A paper by Professor Niels Bohr of Copenhagen, Denmark, title to be announced.

●

Getting Acquainted

To facilitate greater acquaintanceship among physicists, the *Journal of Applied Physics* will publish from time to time pictures of the members of the staffs of university physics departments. This month we present the Physics Department of the University of North Carolina.

OTTO STUHLMAN, JR.
Professor

ARTHUR E. RUARK
Head of Department

KARL H. FUSSLER
Professor

E. K. PLYLER
Professor

PAUL E. SHEARIN
Instructor

JOHN A. WHEELER
Assistant Professor

Innovations in Instruments

Automatic Exposure Camera

Science News Letter reports the issue of a patent to Doctors Gustave Bucky and Albert Einstein on an application of the photoelectric cell to the photographic camera. The essential mechanism is very simple. The current from the photo-cell operates a sector in the form of an optical wedge placed between the elements of the lens. The more light that reaches the photo-cell the greater is the absorption of the part of the wedge between the lens. By this means the radiation reaching the plate may be kept practically constant under large changes in illumination of the object being photographed.

Photo-cell Correction Filter

A new all-glass filter which brings the spectral sensitivity of the photronic photoelectric cell into close agreement with that of the human eye has just been announced by the Weston Electrical Instrument Corporation, Newark, New Jersey. Known as the Viscor (visual correction) filter, it is to be used for critical photometric work based on the international standard eye sensitivity curve, particularly with monochromatic light and other light sources whose output the eye has great difficulty in comparing by using the ordinary photometric methods. The filter, being of glass, is permanent and practical under both laboratory and commercial conditions.

The new filter is intended for use with direct-reading photometric instruments in which a photronic cell converts light energy to electrical energy, and the cell current so produced actuates the indicating meter. While the unfiltered cell responds throughout the entire visible spectrum, it is also affected to some extent by radiant energy in the ultraviolet and near infrared regions which the eye never "sees," and is also more sensitive in its blue and green regions than the eye. Use of the new filter over the cell, however, eliminates the effect of this nonvisible radiant energy and excess sensitivity in the visible spectrum, and maintains cell response approximately proportional to eye response at any wave-length or combination of wave-lengths. The total transmission of the Viscor filter is approximately 37.5 percent.

New Atomic Models

At the meetings of the American Association for the Advancement of Science, and associated societies, in Atlantic City, New Jersey, December 28, 1936, to January 2, 1937, considerable interest was shown in the exhibit of the fertilizer research division, Bureau of Chemistry and Soils, U. S. Department of Agriculture. Electron diffraction photographs taken by Doctors L. R. Maxwell and S. B. Hendricks of the clay minerals kaolinite ($Al_2O_3.2SiO_2.2H_2O$), montmorillonite ($(Mg.Ca)O.Al_2O_3.4\text{–}5SiO_2.nH_2O$) and halloysite ($Al_2O_3.2SiO_2.nH_2O$) were exhibited together with models showing the spacial atomic arrangements for kaolinite and montmorillonite. These two structures are the most common types found in the inorganic colloidal constituents of soils. Swelling of montmorillonite in water can be explained from the knowledge of its crystal structure. The models shown were made by John Rannells, 174 W. 89th St., New York, New York, who has introduced a new method of construction in which colored strips of celluloid are formed into spheres to represent atomic configurations.

Routine Control Spectrograph

The spectrograph is now being used by the Campbell, Wyant and Cannon Foundry Company of Muskegon, Michigan, for the control of the routine production of alloy cast iron. It is used to determine the percentage composition of six elements: Cr, Cu, Mn, Mo, Ni and Si. Two operators make the forty-eight analyses necessary for eight different kinds of iron in thirty-five minutes. The technique of analysis and the control procedure was developed at the physics laboratory of the University of Michigan by Professor Ralph A. Sawyer and Dr. H. B. Vincent under the auspices of the department of engineering research of the university. A complete description of the methods will be published in a later issue of the Journal.

New Impulse Generator

In the General Electric laboratories, a new impulse generator has been built using Pyranol-filled capacitors. The charging voltage and polarity can be adjusted from the oscillograph control board, and a set of switches is provided to adjust easily the generator capacitance. The design provides great flexibility. The several combinations which can be readily obtained are: 1.75 mf in steps of 0.04 mf at any voltage up to 150 kv; 7.00 mf in steps of 0.16 mf at any voltage up to 75 kv; and 28.0 mf in steps of 0.64 mf at any voltage up to 37.5 kv. The maximum stored energy is 19,700 watt-seconds, which can be obtained at any of the tabulated values of maximum voltage. Using the 37.5 kv connection, 60,000 amp. can be supplied.

Glyptal Printing Rollers

Glyptal is now being used by roller manufacturers for printing rollers that will maintain a constant degree of softness and not lose shape during a year or more of constant use. In the early development of glyptal resin,

only hard rollers could be produced. Gradually, as information about the nature of the material came to light, softer and softer rollers were made until, at present, glyptal can be prepared with any degree of flexibility from celluloid-like rigidity to extreme flaccidity. Rollers of this new type are oil resistant and therefore do not swell with printing inks. Furthermore, they do not melt nor become excessively soft during the heat of long summer runs as do ordinary glue-glycerin rollers.

Capillary Lamps

Water-cooled capillary lamps, although not available commercially, are in use in the General Electric laboratory. A 600-watt lamp produces 30,000 lumens from an arc gap one centimeter long, and the brightness of the center core of the arc was 40,000 candles per sq. cm. The light is of good color and the spectrum reveals considerable red radiation. The water-cooled lamp operates on a high reactance transformer.

High Temperature Steam Turbines

Westinghouse Technical Press Service announces the new manufacture of high pressure, high temperature turbines turning hydrogen-cooled generators. Steam temperatures, up to almost 500 degrees centigrade, are now being used. One of these turbine generator units will be installed in New York. It consists of two turbines in tandem driving a single 50,000 kilowatt hydrogen-cooled generator at 3600 r.p.m. The generator rotor will turn in an atmosphere of sealed-in hydrogen and thus its losses have been reduced by 10 percent.

Still Another Use for Photo-cells

Here is a new application of electronic tubes. A motor bus at the end of the run may now roll into a garage covered with mud and, as it moves slowly down the corridor, water sprays from different directions suddenly appear and several pairs of revolving brushes start briskly wiping off the muck, deftly following the contours of the steel sides and the windows. In a few seconds the bus has passed through the section, water is shut off and the bus is spic and span again. The driver hasn't left his seat, no workmen are in evidence. Photoelectric tubes, of course, stand at the water and brush controls and handle the whole job.

Miniature X-ray Equipment

Engineers have steadily improved and simplified x-ray equipment in recent years. Now a new model is announced, capable of developing 90,000 volts, which can be carried around in three small zipper bags.

High Current Circuit Breaker

In a compact multibreaker load center recently developed by Square D and Westinghouse engineers, the circuit interrupting mechanism for each of the poles is made so that, if necessary, each contact can interrupt 5000 amperes at 115 volts, 60 cycles, without objectionable noise or flash. The new unit is a low priced substitute for 115/230 volt a.c. fuse and switch combinations and conventional individual a.c. circuit breakers handling branch circuits from 15 to 35 amperes.

Apparatus Booklets Received Recently

Engineering Properties of Monel. In this 8-page booklet, the following properties of the nickel-copper alloy known as "monel" are given: physical constants, strength and ductility, corrosion resistance, shapes and sizes available, working properties and composition. Obtainable from the Development and Research Division, The International Nickel Company, Inc., 67 Wall Street, New York, New York.

General Radio Experimenter. The December issue describes an audiofrequency Schering bridge and a redesign of the type 200 B Variac. May be obtained from General Radio Company, 30 State Street, Cambridge A, Massachusetts.

Thermal Compressor for Pressures Up To 1000 Atm. P. J. Kipp and Zonen, Delft, Holland.

Colorimeter and Apparatus Dielectric Constant Measurement. A two-page sheet describing a colorimeter of the Dubosq type and a six-page leaflet describing apparatus for reducing measurements of the dielectric constant and dipole moments to a few simple manipulations. P. J. Kipp and Zonen, Delft, Holland.

Vacuum Thermocouples. P. J. Kipp and Zonen, Delft, Holland.

Photoelectric Cells and Apparatus. A description of the Lange type "photoelectric colorimeter," transparency and blackening meter, "gloss meter," "multiflex" galvanometer, photoelectric cells, and other equipment. Published by Pfaltz and Bauer, Inc., Empire State Building, New York, New York.

Have You Read?

The Theory of the Properties of Metals and Alloys. N. F. MOTT AND H. JONES. Pp. 324+xiii, Figs. 108. Oxford University Press, New York, 1936. Price $8.00.

In this book, the authors aim to give an account of the present state of the electron theory of metals and of its successes in accounting for the observed properties of metals and alloys. Quantitative calculations of the metallic bond for the simplest metals and detailed predictions of crystal structure and of the phase equilibrium diagrams of alloys are fully described. In addition the subject matter includes an account of the specific heats and thermal expansion of solids; of the optical properties and color of metals and alloys; and of their magnetic properties and electrical conductivity.

Thermodynamic Properties of Steam. JOSEPH H. KEENAN AND FREDERICK G. KEYES. Pp. 90+large chart. John Wiley & Sons, Inc., New York, 1936. Price $2.75.

The essential data on steam and other phases of water are now considerably increased due to the practical completion of the various series of investigations in progress. The present tables are computed from entirely new formulations of the properties of water. For the most part the formulations are analytical, but graphical methods are employed for volumes less than 10 cubic centimeters per gram. Most of the vapor data are taken from the Keyes equation of state, which yields a higher degree of consistency and precision than the graphic method used in earlier tables. In addition to the tables themselves, the book includes a quite complete sketch of the data sources and mode of correlation, together with an outline of the methods used to prepare the tables.

Properties of Matter. F. C. CHAMPION AND N. DAVY. Pp. 296+xiv, Figs. 161. (Physics Series edited by E. U. Condon) Prentice-Hall, Inc., New York, 1937. Price $4.50.

This book is intended for advanced students who are already familiar with the elementary concepts of mechanics. Such topics are treated as: the acceleration caused by gravity, the gravitational constant, elasticity, seismic waves, capillarity, surface films, kinetic theory, osmotic pressure, diffusion, viscosity, and errors of measurement. The aim of the author has been to present these subjects from a physical point of view and particularly to avoid regarding the material as exercises in applied mathematics.

Report of the Committee on the Measurement of Geologic Time. ALFRED C. LANE, chairman. Pp. 87 (including several charts and tables). National Research Council. September 1936.

Statistical Mechanics—The Theory of the Properties of Matter in Equilibrium. R. H. FOWLER. Second edition. Pp. 864+x, Figs. 101. The University Press, Cambridge, and The Macmillan Co., New York, 1936 (Released January 5, 1937). Price $14.00.

This is a second edition of Professor Fowler's monumental book on statistical mechanics. Quantum statistics, which was relegated to a concluding chapter in the first edition, has been incorporated into the exposition from the start. Sections on ferromagnetics and semiconductors have been added. Most of the changes consist, however, of revision and modernization of both theory and experiment. Professor Fowler concludes with, "Since the last sentence of the first edition was written statistical mechanics has changed greatly. Then it was the general theory which was emerging clarified from quantum mechanics. This process seems now to be complete, and further developments likely to consist mainly of more complicated and more widely ramifying applications, similar to those treated here."

Textbook on Spherical Astronomy. W. M. SMART. Second edition. Pp. 430+xii, Figs. 149. University Press, Cambridge, and The Macmillan Co., New York, 1936. Price $5.50.

This work is intended to fill the gap between spherical astronomy, as presented by older textbooks, and the position now achieved by the aid of modern developments. In addition to the usual problems this book contains discussions of heliographic coordinates, proper motions, determination of position at sea, the use of photography in precise astronomical measurements, the orbits of binary stars, and, in the second edition in the appendices, method of dependences, stellar magnitudes and the coelostat.

Applied Radio-Chemistry. OTTO HAHN, director of the Kaiser Wilhelm Institute for Chemistry. Pp. 275. Cornell University Press, 1936. Price $2.50.

Giving the 1933 Baker nonresident lectures. In spite of its title it gives a most interesting resumé of the physics of radioactivity and of its applications in chemical analysis. The book might have been aptly entitled "The Applications of the Physics of Radioactivity to Chemical Analysis."

Metals Handbook. Written by the cooperative effort of many members of the American Society for Metals. Pp. 1392. American Society for Metals, 1936.

Constitution and physical properties of all of the common ferrous and nonferrous metals and alloys. Also, methods of testing, working heat treatment, machining, etc., and general data.

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CURRENT LITERATURE OF PHYSICS

THE Editors present for convenient reference in this section a list of those articles dealing with physics published in some thirty selected journals. Their definition of the scope of physics and their selection of the particular journals to be included are necessarily based on their interpretation of the wishes of the majority of the readers. The emphasis is placed on prompt mention and convenience of use rather than completeness. For many other excellent articles and publications, reference should be made to the regular abstract journals. Miss Margaret Shields, Physics Librarian at Princeton University, very kindly prepares the listings for the foreign language journals.

American Physics Teacher

VOL. 4, NO. 4, DECEMBER, 1936.

Training of Physicists for Industry—From the Point of View of the Educator. pp. 167–174. ***Homer L. Dodge***

Plane and Solid Angles. Their Pedagogic Value When Introduced Explicitly. pp. 175–179. ***J. B. Brinsmade***

Professor James Beebee Brinsmade. 1884–1936. p. 180. ***W. E. McElfresh***

Early History and the Methods of Infrared Spectroscopy. pp. 181–189. ***R. B. Barnes and L. G. Bonner***

High School Physics as a Preparation for College Physics. pp. 190–194. ***L. B. Ham***

Gibbs and Mollier Thermodynamic Surfaces. pp. 194–196. ***M. W. Zemansky and R. C. Herman***

Rational-Sided Right Triangles. pp. 197–198. ***Ralph Hoyt Bacon***

Apparatus, Demonstrations and Laboratory Methods:

Electrically Driven Tuning Fork as a Source of Constant Frequency for the Precise Measurement of Short Intervals of Time. pp. 199–200. ***R. B. Dow***

Intermittent Air-Blast Method of Exciting Transverse Vibrations in a Bar. pp. 201–202. ***B. W. Currie***

Double Bulb Neon Oscillograph. p. 202. ***James F. Koehler***

Some Demonstrations of Spinning Tops and Gyroscopes. pp. 203–204. ***Robert C. Colwell***

Mechanical Vibrator for Demonstrating Standing Waves. p. 205. ***W. J. Jackson and F. R. Pratt***

Determination of the Speed of Sound by the Fizeau Toothed Wheel Method. pp. 206–208. ***Harold K. Schilling***

Free-Fall Apparatus Which Uses Photographic Recording. pp. 209–210. ***Glenn F. Rouse***

Indicating Lantern Slide Color Mixer. p. 211. ***John J. Heilemann***

Continuously Variable Diaphragm for Use in Spherical Aberration Studies. p. 212. ***W. P. Gilbert***

Model of Magnetization. pp. 213–214. ***F. W. Warburton***

Easily Constructed Fresnel Mirrors. pp. 215–216. ***H. M. Reese***

Discussion and Correspondence:

Acceleration Calculation from Spark Recorded Data. p. 217. ***Philip Rudnick***

Text Discussions on Lightning. pp. 217–218. ***J. C. Jensen***

Laboratory Experiments on the Viscosity of Air—An Undergraduate Project. pp. 218–219. ***D. Roller and D. Wooldridge***

Annalen der Physik

SERIES 5, VOL. 27, NO. 7, NOVEMBER 22, 1936.

Magnetic Anisotropy of Single Crystals of Iron and Nickel. pp. 573–596. ***H. Schlechtweg***

Probe Measurements in Low Pressure Mercury Discharge. pp. 597–621. ***W. Denecke***

Preliminary Report on Magnetic After Effect. pp. 622–624. ***H. Wittke***

Magnetic Skin Effect in Ferromagnetic Circular Cylinders for Strong and Weak Alternating Fields. pp. 625–642. ***B. Kyewski***

Zeeman Effect of Alkalis from the Dirac Equation. pp. 643–663. ***R. Schlatterer***

Logarithmic and Linear Sound Intensity Scales. pp. 664–668. ***W. Bürck et al.***

SERIES 5, VOL. 27, NO. 8, DECEMBER 4, 1936.

Interference from Lattices Under X-Ray Excitation. pp. 669–693. ***G. Borrmann***

Fine Structure of Emission of Copper Single Crystal Anticathodes. pp. 694–720. ***H. Voges***

Measurement of Velocity and Energy Distribution of Charged Particles. pp. 721–741. ***R. Kollath***

New Condenser Pressure Gauge. pp. 742–752. ***C. Johansson***

SERIES 5, VOL. 28, NO. 1, DECEMBER 9, 1936.

Specific Heat of Electrons in Metals. pp. 1–10. ***A. Sommerfeld***

Electron Optics of Long Solenoids. pp. 11–20. ***H. Busch***

Ultramicroscopic Roughness of Metal Surfaces and Their Effect on Electron Interference Images. pp. 21–27. ***F. Kirchner***

High Frequency Losses and Semi-Crystalline Structure of Liquids. pp. 28–34. ***P. Debye and W. Ramm***

Absolute Measurements of Dielectric Losses at High Frequency with Condenser Thermometers. pp. 35–53. ***C. Schmelzer***

Nature of Hydrate Binding in Ions of Transition Elements, Specially Co^{+++}. pp. 54–58. ***G. Joos***

Effect of Displacement Current on a.c. Resistance of Simple Circuits. pp. 59–70. ***W. Wessel***

X-Ray "Spark" Lines. pp. 71–86. ***F. Wisshak***

Efficiency of $D \rightarrow D$ Nuclear Reaction. pp. 87–96. ***R. Döpel***

Excitation of Mercury by Collision of Slow Positive Mercury Ions. pp. 97–103. ***H. Moser***

Raman Investigation of Glasses. pp. 104–113. ***R. Langenberg***

Temperature Compensated Bar Pendulum. pp. 114–120. ***O. Meisser***

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Annales de physique

SERIES 11, VOL. 6, DECEMBER, 1936.

Radioactivity of Mountain Air. pp. 751–825. ***H. Garrigue***
Specific Heat of Iron. pp. 826–855. ***Mme. Lapp***
Rotatory Power and Structure of Electrolytic Solutions. pp. 856–993. ***I. Peychès***

Comptes rendus

VOL. 203, NO. 23, DECEMBER 7, 1936.

Definition of Distance in Relativity Theory. pp. 1236–1238. ***J. LeRoux***
Electronic Model of Dirac Wave Mechanics. pp. 1238–1240. ***T. de Donder and J. Gèhéniau***
Relativistic Quantum Mechanics of the Electron. pp. 1240–1242. ***A. Datzeff***
Absorption Spectra of Saturated Acyclic Alcohols 6000–9500A. pp. 1244–1247. ***P. Barchewitz***
Luminescence Phenomena Bearing on Production of Sensibly White Light. pp. 1247–1249. ***M. Servigne***

Journal de physique

SERIES 7, VOL. 7, NOVEMBER, 1936.

Study of Liquid State. pp. 441–447. ***E. Bauer et al.***
Methods of Measuring Explosive Pressures. pp. 448–453. ***A. Langevin et al.***
Permanent Magnetization of Steel by a Rapid Aperiodic Discharge. pp. 453–460. ***R. Chevallier and M. Laporte***
Ultraviolet Radiation in the Reboul Effect. pp. 461–465. ***O. Viktorin***
Coulomb Interaction of Two Neutral Atoms. pp. 466–468. ***L. Goldstein***
Heat of Absorption of Gas by Carbon at −183°A. pp. 469–472. ***R. Lepoint***
Statistical Study of Bundles of Cosmic Rays. pp. 473–475. ***P. Auger and P. Ehrenfest***
Near Infrared Absorption and Raman Spectra of Various Amines. pp. 476–480. ***M. and R. Freymann***

Journal of the Acoustical Society of America

VOL. 8, NO. 3, JANUARY, 1937.

Reverberation and Absorption of Sound. pp. 155–161. ***Erwin Meyer***
Modified Tube Method for the Measurement of Sound Absorption. pp. 162–171. ***Keron C. Morrical***
Theory of Sound Transmission Through Porous and Nonporous Materials. pp. 172–175. ***Michael Rettinger***
Positions of Stimulation in the Cochlea by Pure Tones. pp. 176–180. ***John C. Steinberg***
Developments in Aircraft Sound Control. pp. 181–184. ***Henry H. Bruderlin***
Scale for the Measurement of the Psychological Magnitude Pitch. pp. 185–190. ***S. S. Stevens et al.***
On Hearing by Electrical Stimulation. pp. 191–195. ***S. S. Stevens***
Theory of the Haskell Organ Pipe. pp. 196–198. ***Arthur Taber Jones***
Strike Note of Bells. pp. 199–203. ***Arthur Taber Jones***

Journal of the Franklin Institute

VOL. 223, NO. 1, JANUARY, 1937.

On Gravitational Waves. pp. 43–54. ***A. Einstein and N. Rosen***
Impulse Circuits for Obtaining a Time Separation Between the Appearance of Potential at Different Points in a System. pp. 55–76. ***L. B. Snoddy et al.***
Derivation of Coefficients of Orifices. pp. 83–94. ***W. E. Howland and J. D. Richetta***

Journal of the Optical Society

VOL. 27, NO. 1, JANUARY, 1937.

Search for High Efficiency Light Sources. pp. 1–24. ***Saul Dushman***
Problems Concerning the Production of Cathode-Ray Tube Screens. pp. 25–35. ***H. W. Leverenz***
Visibility of Radiation at Low Intensities. pp. 36–43. ***K. S. Weaver***
Optics and the Textile Industry. pp. 44–50. ***Edward R. Schwarz***
Biprism for Use in Stellar Interferometry. pp. 51–54. ***Edward M. Thorndike***

Nature

VOL. 138, NO. 3502, DECEMBER 12, 1936.

Letters to the Editor:

Spectral Absorption and Fluorescence of Dyes in the Molecular State. pp. 1009–1010. ***Edwin E. Jelley***
Beilby Layer on Non-Metals. pp. 1010–1011. ***G. Ingle Finch***
Scattering of Neutrons by Protons. p. 1011. ***E. T. Booth and C. Hurst***
Inconsistency of the Neutrino Theory of Light. p. 1011–1012. ***V. Fock***
Minimal Lines and Geodesics within Matter: a Fundamental Difficulty of Einstein's Theory. p. 1012. ***Ludwik Silberstein***
Colloids and the Biological Effect of Radiation. p. 1014. ***F. Ellinger***
Radio Fadings and Bright Solar Eruptions. p. 1017. ***H. W. Newton***

VOL. 138, NO. 3503, DECEMBER 19, 1936.

Letters to the Editor:

Mechanism of Chemical Reaction in the Electric Discharge. p. 1054. ***E. J. B. Willey***
Continuous Absorption Band of Rubidium in the Presence of Foreign Gases. p. 1055. ***Ny Tsi-Zé and Ch'en Shang-Yi***
Effects of Chemical Combination with Oxygen and Fluorine on the $K\alpha_{1,2}$-Doublet of Some of the Lighter Elements. p. 1056. ***N. G. Johnson***
Mechanism of Carbohydrate Oxidation. p. 1057. ***F. Dickens***
Some New Phenomena Produced by Sound Vibrations. p. 1059. ***F. L. Hopwood***
Adjustable Resonators and Orchestration. p. 1059. ***W. A. Osborne***
Directions of Homogeneous Auroral Arcs. p. 1059. ***R. A. Hamilton***

VOL. 138, NO. 3504, DECEMBER 26, 1936.

Letters to the Editor:

Isotopic Weights by the Doublet Method. p. 1094. ***F. W. Aston***
Selective Absorption of Neutrons in Silver. p. 1094. ***E. H. S. Burhop et al.***
Activation of Silver by Neutrons. p. 1095. ***Georges Guében***

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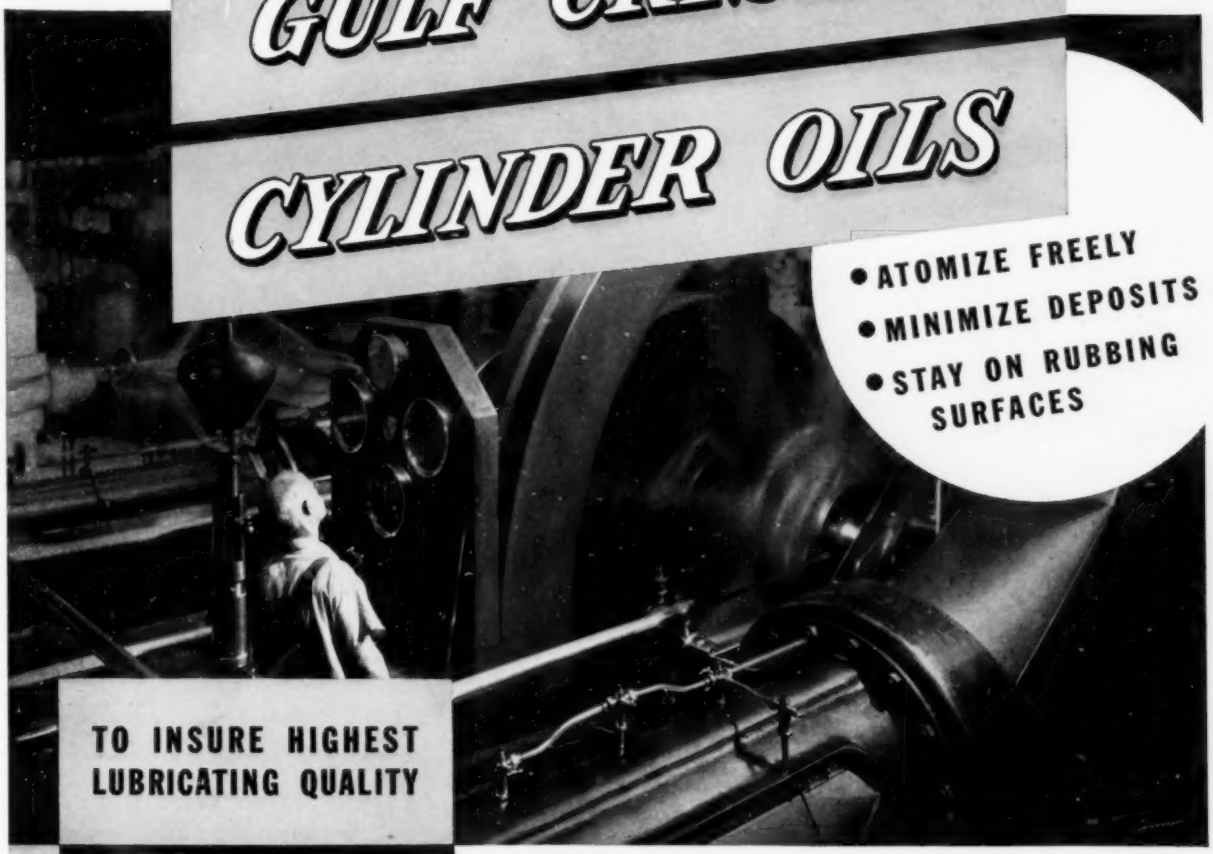

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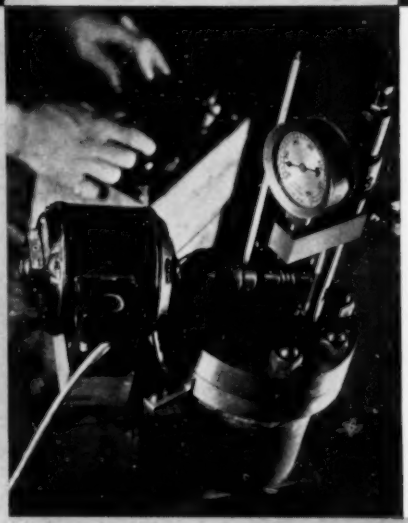

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